THE CLAIM OF LEIBNITZ

TO THE

INVENTION OF THE DIFFERENTIAL CALCULUS.

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THE CLAIM OF LEIBNITZ

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OF THE

DIFFERENTIAL CALCULUS,

BY

DR. H. SLOMAN.

TRANSLATED FROM THE GERMAN WITH CONSIDERABLE ALTERATIONS AND NEW ADDENDA BY THE AUTHOR.

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To

SIR DAVID BREWSTER,

THE

BIOGRAPHER OF NEWTON,

AND THE

REV. J. EDLESTON,

This Volume,

IS RESPECTFULLY DEDICATED

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THE AUTHOR.

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GERHARDT: "So it is Leibnitz —— "to whom we owe this. The Chapter "in which the question respecting the "first discoverer of the higher Analysis "has till now been agitated, now disap- "pears from the history of the mathe- "matical sciences. The battle of more "then one hundred years, about the first "discoverer of the Differential Calculus, "is now at its end." *Gerhardt* II. page 62.

GERHARDT: "So ist es Leibnitz — "—— dem wir dies verdanken. Das "Capitel, in welchem die Frage über "den ersten Entdecker der höheren Ana- "lysis bisher erörtert wurde, verschwindet "fortan aus der Geschichte der mathema- "tischen Wissenschaften. Der mehr als "hundert jährige Streit über den ersten "Entdecker der Differenzialrechnung ist "zu Ende." *Gerh. Abh.* II. S. 62.

EDLESTON: Synoptical view of Newton's life:

1666 Octob. Small tract on fluxions and fluents with their applications to a variety of problems on tangents, curvatures, areas, lengths, and centres of gravity of curves.

1666 November. Small tract similar to the preceding but apparently more comprehensive. (Notation by points in first and second fluxions, Basis of his larger tract of 1671.) *Edleston* in his Correspondence of Sir I. Newton, 1850, page 21.

LEIBNITZ: Cum Parisios apulissem anno Christi 1672 eram ergo —— in pene dixerim superba Matheseos ignorantia. *Leibnitz* in Gerhardt's Pamphlet, I, page 29 et 30, line 2.

CHAPTER I.

BARROW AND THE METHOD OF TANGENTS.

From about the year 1650, the vigorous mathematical life, in which England had never been deficient, is seen to receive there an extraordinary impulse, and attain to such a degree of development, that that country became the centre of all the mathematical activity of the period, while in France, after the death of Descartes, there are no important men to name in mathematics.*

* Perhaps even Descartes was much indebted to the English Harriot. For not only does the upright Wallis, who would never knowingly have uttered an untruth, affirm this with zealous warmth in many passages of his *Tractatus Algebræ historicus et practicus*, but it was also believed by contemporaries, and at the same time countrymen of Descartes's, who are spoken of in Baillet's *Vita Cartesii*, and by Roberval, *qui s'entretenant un jour avec Milord Cavendish, lui témoignant être inquiet, d'où était venu à Descartes l'idée, d'égaler tous les termes d'une équation à zéro, Milord Cavendish lui dit, qu'il n'ignorait cela que parcequ'il était Français, et lui offrit de lui montrer le livre auquel Descartes devait cette invention. En effet il le mena chez lui, et lui montra l'endroit de Harriot, où l'on voit la même chose; sur quoi Roberval, transporté de joie, s'écria, "il l'a vu, il l'a vu!" et il le publia de toute part.* We quote this out of Montuclat. II., p. 144. When Colbert in 1666 was looking about him for men, out of whom to form an *Académie des Sciences*, he found no geometers or astronomers in France, except the following: viz., Auzout, Buot, Carcavi, Couplet, Frenicle, Niquet, Picart, Richer, Roberval and De la Voye—none of them, with the exception of Roberval, who died soon after, persons of any great eminence. It was on them and their immediate successors that Leibnitz and Bernouilli, who were both their colleagues, pronounced the following judgment: (See Gerhardt's edition of the Math. Works of Leibnitz, p. 814: the earlier editions

Two problems occupied at that time the attention of geometers, namely the problem of Tangents and that of Quadratures, in which Barrow and Wallis, in England, had achieved the most advanced positions.

The two problems had as yet no mutual connexion; for the object contemplated was measure in one of them, and direction in the other. It will be readily understood, that Barrow's method of tangents cannot be left unnoticed in an enquiry like ours; and so indeed a great deal is said respecting him, by the most modern writers in France and Germany—as Biot in the "Journal des Savants," and Gerhardt in his various writings—who have aroused in the present day a lively interest in the question, was Leibnitz the discoverer of the Differential Calculus, and to what extent?

We need not on this point speak at much length. Barrow says,* *Nulla est magnitudo, quæ non innumeris modis intelligi producta possit,*

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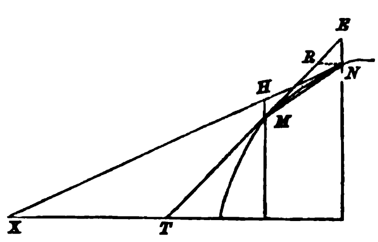
of the Correspondence do not contain this passage) *Verissimum est, quod de nonnullis Academicis notas—et sane quæ a se habent plerumque sunt mediocria, ne dicam ridicula—et si quid boni edunt, dubitare non licet, quin ab aliis furati sint.*

* Compare p. 15 of his principal work, and the one which made the greatest noise at that time, entitled, *Lectiones Geometricæ in quibus præsertim generalia curvarum symptomata declarantur.* Of this work the date is not without importance: it was published in 1670, (and not in 1674, as Gerhardt says in his tract of 1848, p. 15—nor yet in 1672, as he supposes in his tract of 1855, p. 45). That Leibnitz before his discovery of the Differential Calculus either in 1676 or in 1675 or in 1674, should not have read this work, (as Gerhardt affirms in the place quoted,) is inconceivable. Books were not so abundant in those times. Indeed evidences to the contrary are contained in the documents, which Gerhardt himself produces. In App. 1, to Gerhardt's tract of 1848, p. 32, Leibnitz says expressly, that he had seen from Barrow's Lectiones "cum prodirent"—what they contained. This proves that Leibnitz possessed Barrow's book not long after its first appearance, 1670. Gerhardt gives another document, (Tract of 1855, p. 129,) from which the same conclusion may be drawn. This document is, as Gerhardt affirms, dated in Leibnitz' hand-writing 1 Nov., 1675, and therein we have again Leibnitz' own words: *Pleraque theoremata Geometriæ indivisibilium, quæ apud Cavallerium, Vincentium, Gregorium, Barrovium, extant, etc.*

per motus locales, per intersectiones magnitudinum, per quantitate portioneque determinatas ab assignatis locis distantias, per ductus magnitudinum in magnitudines, per applicationes magnitudinum ad magnitudines, per aggregationem magnitudinum ordine certo dispositarum, per appositionem magnitudinum ad alias, vel subductionem ab aliis. Horum modus primarius, et quem alii omnes quodammodo supponant oportet est iste per motum localem. In spite of this idea, which involves his peculiar mode of contemplating the subject, Barrow is entirely devoted to the more important method of Cavalleri, who considers every figure as composed of parts infinitely minute and numerous, and every curve of an infinity of straight lines. So he says, for example, at p. 15: *curva aliquis, vel e rectis (angulos efficientibus) composita, quae curvae quoque nomen merito ferat; Archimedes enim e rectis compositas lineas (uti figurarum circulis inscriptarum perimetros) καμπυλῶν γραμμῶν nomine complectitur, ut et vicissim curvae quaevis lineae censeri possunt e rectis innumeris quidem illis indefinite parvis adjacentibus et deinceps secum angulos efficientibus, confectae.* So likewise, in Lectio II. §. 21: *curva quaedam superficies, circularibus quasi peripheriis constans, (Atomistarum enim phrasin facilitatis, perspicuitatis, brevitatis, addere licet et verisimilitudinis, causa non illibenter usurpo).* And *modus—dimensiones investigandi juxta methodum indivisibilium, omnium expeditissimam, et modo rite adhibeatur haud minus certam et infallibilem.**)

We owe it to Gerhardt that attention has been again directed to this side of the Idea of Barrow.



Now in the 4th Lectio, p. 40, Barrow proves a very important proposition for the application and drawing of tangents to figures according to this method. He says: in figurâ 26 *tangant rectae TM, XN, dico curvae arcum MN recta NH majorem esse;*

* Compare also p. 21 *ad fin.*; we quote the edition of 1670.

recta vero ME minorem. For draw the chord *MN*, and draw *NR* parallel to *XT*, then *NHM* shall be an obtuse angle, and therefore the chord *MN* and *à fortiori* its arc shall be greater than *HN*. On the other hand, because *RNE* is a right angle, *RE* shall be greater than *RN*, and therefore $ME > MR + RN$; but according to Archimedes, (*ex Archimedæis assumptis*) $MR + RN$ (the polygon circumscribed about a curve,) is already greater than the curve that is inscribed in it; therefore the arc *MN* will be less than *ME*. *Perutilis*, says Barrow, *est hæc propositio in tangentium demonstrationibus expediendis. Etenim hinc consectatur, si arcus MN indefinite parvus ponatur, ejusce loco alterutram tangentis particulam ME vel NH tuto substitui.*

Lectio X. begins with these words: *Institutum circa tangentes negotium adhuc urgeo*, and when the theorems which it remained necessary to supply on the subject of tangents, have been exhibited in proper geometric form, we read on page 80:* *Ita propositi nostri* (*priore, quam innuebamus parte*) *quodammodo defuncti sumus. Cui supplendae, appendiculae instar, subnectemus a nobis usitatam methodum ex Calculo tangentes reperiendi. Quamquam haud scio, post tot eiusmodi pervulgatas atque protritas methodos, an id ex usu sit facere. Facio saltem ex Amici*† *consilio; eoque lubentius, quod prae ceteris, quas tractavi, compendiosa videtur, ac generalis. In hunc procedo modum.*

Sint AP, PM positione datae lineae et MT curvam tangere ponatur, rectae PT quantitatem exquiram; curvae arcum MN indefinite parvum statuo; nomino $MP = y$; $PT = t$; $MR = a$; $NR = e$; *ipsas MR, NR* (*et mediantibus illis ipsas MP, PT*) *per aequationem e Calculo deprehensam inter se comparo; regulas interim has observans* 1, *inter computandum omnes abjicio terminos, in quibus ipsarum a, vel e potestas*

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* We quote verbatim, except that we call the abscissa and its ordinata x and y, and not with Barrow f and m.

† Weissenborn in his "Beitrag zur Geschichte der Mathematik oder Principien der höheren Analysis," Halle 1856, takes for granted that Newton is the friend here intimated.

habetur vel in quibus ipsae ducuntur in se (etenim isti termini nihil valebunt).

2. *Post aequationem constitutam omnes abjicio terminos, literis constantes quantitates notas, seu determinatas designantibus; aut in quibus non habentur a, vel e (etenim illi termini semper ad unam aequationis partem adducti, nihilum aequabunt).*

3. *Pro a ipsam y (vel MP) pro e ipsam t (vel PT) substituo. Hinc demum ipsius PT quantitas dignoscetur.*

Quod si calculum ingrediatur curvae cujuspiam indefinita particula; substituatur ejus loco tangentis particula rite sumta; vel ei quaevis (ob indefinitam curvae parvitatem) aequipollens recta.

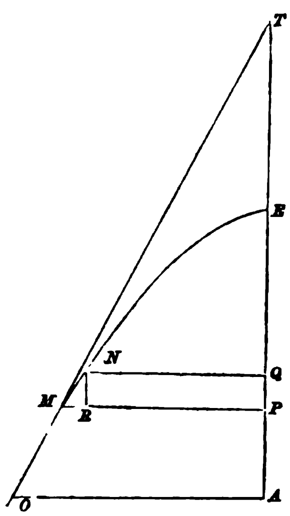
Haec autem e subnexis Exemplis clarius elucescent.

Exempl. Sit recta EA positione ac magnitudine data et curva EMO proprietate talis, ut ab ea utcunque ductâ rectâ MP ad eam perpendiculari summa cuborum ex AP et MP aequetur Cubo rectae AE; $x^3+y^3=r^3$. *(Fiant quae praescripta sunt) Nominatis* $AE=r$; $AP=x$; (and as before $MP=y$; $PT=t$; $MR=a$, $NR=e$); *unde*

$AQ=x+e$, *et* AQ *cub* $=x^3+3x^2e+3xe^2+e^3$ *(seu rejectis uti monitum est rejiciendis)* $=x^3+3x^2e$.

Item NQ *cub* $=$ *cub* $(y-a)=y^3-3y^2a+3ya^2-a^3$ *(hoc est)* $=y^3-3y^2a$.

Quapropter est $x^3+3x^2e+y^3-3y^2a=$ (AQ *cub* $+NQ$ *cub* $=AE$ *cub* $=$) r^3 *abjectisque datis est* $x^2e-y^2a=O$ *seu* $x^2e=y^2a$ *subrogatisque loco a et e ipsis y et t erit* $x^2t=y^3$; *seu* $t=\frac{y^3}{x^2}$.

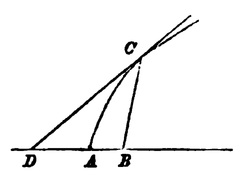


That is, the equation of the curve $y^3+x^3=r^3$ gives by this mode of calculation—which at first assumes infinitely small increments MR and NR, a and e, which afterwards again vanish—the expression for the sub-tangent $t=\frac{y^3}{x^2}$.

This process Barrow illustrates by four further examples, and at page 84 he passes on with the words, *Hæc sufficere videntur huic methodo illustrandæ*, to other geometrical investigations, that is to *Lectio XI.*, which begins with the words; *Reliquis utcunque paratis, apponemus jam quæ ad magnitudinem e tangentibus seu e perpendicularibus ad curvas dimensiones eliciendas pertinentia se objecerunt theoremata.* Then follows Barrow's geometrical method of Quadratures, of which we shall not speak at present.

We repeat therefore that Barrow's Method of Tangents of 1670, which Leibnitz had read, consisted in applying to the problem of Tangents the idea of neglecting the higher powers of infinitely small quantities.

When Sluse, some time after Barrow, proposed a more convenient rule for the expression of tangents, Newton wrote, that he also had a rule for tangents, which was peculiarly suitable for quadratures; this rule Newton gives in his well-known letter of the 10th Dec., 1672, about which there has been so much controversy, some affirming that Leibnitz was acquainted with it, and others that he was not. *Ex animo gaudeo* (writes Newton to Collins, 10th Dec. 1672) *D. Barrovii amici nostri Rev. Lectiones mathematicas exteris adeo placuisse, neque parum me juvat intelligere eos in eandem mecum incidisse ducendi Tangentes Methodum. Qualem eam esse conjiciam ex hoc exemplo percipies. Pone CB applicatam ad AB, in quovis angulo dato, terminari ad quamvis curvam AC, et dicatur* $AB = x$ *et* $BC = y$, *habitudoque inter* x *et* y *exprimatur qualibet aequatione, puta* $x^3 - 2xxy + bxx - bbx + byy - y^3 = 0$, *qua ipsa determinatur Curva. Regula ducendi Tangentem haec est; multiplica aequationis terminos per quamlibet progressionem arithmeticam juxta dimensiones* y, *puta* $\underset{0}{x^3} - \underset{1}{2xxy} + \underset{0}{bxx} - \underset{0}{bbx} + \underset{2}{byy} - \underset{3}{y^3}$; *ut et juxta dimensiones* x, *puta* $\underset{3}{x^3} - \underset{2}{2xxy} + \underset{2}{bxx} - \underset{1}{bbx} + \underset{0}{byy} - \underset{0}{y^3}$. *Prius productum erit Numerator, et posterius divisum per* x *Denominator Fractionis, quae exprimet longitu-*



dinem BD, ad cujus extremitatem D ducenda est Tangens CD: est ergo longitudo $BD = \dfrac{-2xxy + 2byy - 3y^3}{3xx - 4xy + 2bx - bb}$.

Hoc est unum particulare, vel corollarium potius Methodi generalis, quae extendit se, citra molestum ullum calculum, non modo ad ducendum Tangentes ad quasvis Curvas, sive Geometricas, sive Mechanicas, vel quomodocunque rectas lineas aliasve Curvas respicientes; verum etiam ad resolvendum alia abstrusiora Problematum genera de Curvitatibus, Areis, Longitudinibus, Centris Gravitatis Curvarum, etc. Neque (quemadmodum Huddenii methodus de Maximis et Minimis) ad solas restringitur aequationes illas, quae quantitatibus surdis sunt immunes.

Hanc methodum intertexui alteri isti, qua Aequationum Exegesin instituo, reducendo eas ad Series infinitas. Memini me ex occasione aliquando narrasse D. Barrovio, edendis Lectionibus suis occupato, instructum me esse huiusmodi methodo Tangentes ducendi: Sed nescio quo diverticulo ab ea ipsi describenda fuerim avocatus.

Slusii Methodum Tangentes ducendi brevi publice prodituram confido: quamprimum advenerit exemplar ejus, ad me transmittere ne grave ducas.

On reading this letter in the present century, we are impelled to ask for the demonstration, but in the year 1672 every one knew from the mention of the Huddenian Method *de Maximis et Minimis*, (which was based upon and proved by an infinitely small increment to the abscissa), and furthermore from the frequent mention of Barrow, that the foundation and proof of this Newtonian Method of Tangents lay also in that infinitely small increment.

We see that Newton's letter commends this method as a universal one, while Barrow's rule, it is said, did not yet embrace all cases as bearing on one another; at the same time the letter says, that Newton's rule is a corollary to his method of Quadratures.

We shall hereafter return to this letter, but will previously obtain a clear insight into the method employed at that period for Quadratures.

CHAPTER II.

WALLIS AND THE PROBLEM OF QUADRATURES.

With the invention of the Differential Calculus Wallis has a more direct connexion than even Barrow, and it is not by mere accident that his two contemporaneously published *Tractata de conicis sectionibus* and *Arithmetica infinitorum* exhibit the first steps in that direction, for he makes express reference to what still remained to be done, and has since actually been done by the Differential Calculus.* His labours occupy the very same period, in which the discovery took place. We take the course of his progress from the preface to the *Arithmetica infinitorum* and to the treatise *de sectionibus conicis*, (published at the same time, and to which he refers in the *arithmetica infinitorum*). *Opus hoc*, he says, *plane novum. Exeunte anno* 1650 *incidi in Toricelli scripta*,

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* His principal work *arithmetica infinitorum* is a well-known one; it appeared in the year 1655, and its title in full was, *ar. inf. sive nova methodus inquirendi in curvilineorum quadraturam.* Barrow had handled the subject of quadratures and tangents in the main geometrically, and had made a mere adjunct of his analytical method of calculating, because he had a less warm attachment to arithmetic and algebra. *Appendiculæ instar*, (compare p. 4), says he, when he has already handled the theory of tangents in a geometrical form, *subnectemus à nobis usitatam methodum ex calculo tangentes reperiendi*, and immediately he goes back again to the forms of geometry, in order to investigate the *Dimensiones curvarum ex tangentibus seu e perpendicularibus ad curvas.* Wallis on the contrary had a greater partiality and respect for calculations, for arithmetic, and in consequence came nearer to the Differential Calculus than Barrow.

ubi Cavalleri Geometriam Indivisibilium exponit. Ipsius methodus, Wallis continues, *mihi quidem eo gratior erat, quod nescio quid eiusmodi ex quo primum fere Mathesin salutaverim, animo observabatur.*

Ubi huiusmodi jam obtinuisse methodum persenseram cogitare apud me coepi, num non hinc aliquid de circuli Quadratura, quam summos semper viros exercuisse notum est, luminis accedat. Quod spem facere videbatur, hoc erat. Infinitorum Coni circulorum, ad totidem Cylindri, ratio jam erat cognita, nempe ut 1 *ad* 3.

Manifestum etiam erat rectas trianguli esse Arithmetice proportionales, sive ut 1, 2, 3 *etc. ergo circulos coni (in diametrorum ratione duplicatâ) ut* 1, 4, 9 *etc.*

Hoc autem si universali aliquâ methodo invenire possem, de Circuli Quadraturâ satis prospectum esset. Reducto ita problemate Geometrico ad pure Arithmeticum.

Aggressus igitur sum primo (ut a simplicioribus inchoarem) series simplices. Adeoque hinc statim Geometriam auctam persensi; cum enim antea ex figuris curvilineis sola fere Parabola quadraturam nacta erat, jam Paraboloeidium omnium infinita genera unâ quidem et generali methodo unica propositio quadranda doceat.

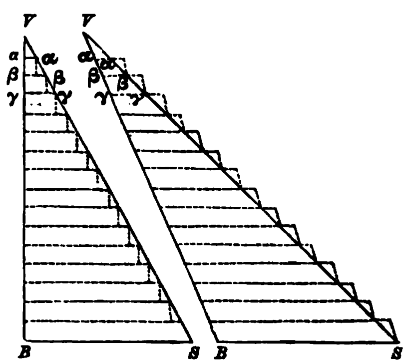
Transii deinde ad series auctas (quas voco) et diminutas sive mutilatas; quae ex duarum pluriumve serierum vel aggregatis aut differentiis constant. Atque hic etiam successum minime contemnendum reperi. Nempe eas omnes ad series aequalium redigere non erat difficile, adeoque Conoeidea et Spheroeidea, vel etiam Pyramidoeidea, non modo recta sed inclinata, ad Cylindros et Prismata redigere rem nullius esse negotii perspexi.

De seriebus autem istis sive auctis sive diminutis non ipsis solum, sed et quae in earum ratione duplicatâ, triplicatâ, aut ulterius multiplicata procedunt, eandem inquisitionem eodem successu continuavi, uti ex iis quae deinceps sequuntur propositionibus videndum est. Ubi simul numerorum figuratorum, puta triangularium, pyramidalium etc., (quorum nullus vel exiguus hactenus fuerat usus et fere ludicrus), usus insignes ex insperato deteguntur.

Verum ubi de seriebus aliis quae sint in istarum auctarum vel diminutarum ratione subduplicatâ, et subtriplicatâ etc., agendum erat, quod Circuli, Ellipseos, et Hyperbolae quadraturam directe quidem et immediate spectabat, et quae sola jam superfuit difficultas: videbam illic aquam haerere. He has therefore, he intimates, proposed a problem on that subject to geometricians.

It was this problem that afterwards gave a more direct occasion to the discovery of the Differential Calculus.

In the treatise itself Wallis says: *Suppono in limine (juxta Bonaventurae Cavallerii Geometriam Indivisibilium) Planum quodlibet quasi ex infinitis lineis parallelis constare: Vel potius (quod ego mallem) ex infinitis Parallelogrammis aeque altis; quorum singulorum altitudo sit totius altitudinis* $\frac{1}{\infty}$ *sive aliquota pars infinite parva, adeoque omnium simul altitudo aequalis altitudine figurae. Propositio II. Si triangulum rectis basi parallelis secetur, erunt abscissa triangula secto triangulo similia, et propterea latera habebunt proportionalia (ut notum est). Adeoque—rectae* $\alpha\alpha$, $\beta\beta$, $\gamma\gamma$ *etc.—propter aequales excessus* $V\alpha$, $\alpha\beta$, $\beta\gamma$ *etc. erunt arithmetice proportionales hoc est ut* 1, 2, 3 *etc.—Ideoque si rectae illae supponantur numero infinitae, erit—totum Triangulum aggregatum rectarum numero infinitarum, quarum minima est punctum, maxima est BS, Basis Trianguli.*



Prop. III. De area Trianguli. Itaque cum Triangulum constet ex infinitis sive lineis, sive Parallelogrammis, arithmetice proportionalibus, a puncto inchoatis et ad basin continuatis: Erit Area Trianguli aequalis Basi in Altitudinis semissem ductae.

Est enim notissima apud Arithmeticos regula: Summam Arithme-*

* Here we have the idea of summation.

ticae progressionis, sive omnium quotcunque terminorum aggregatum, aequari aggregato extremorum in semissem numeri terminorum ducto. Nam si terminus minimus supponatur 0 (*prout hic supponitur*) *idem erit extremorum aggregatum atque ipse terminus maximus. Altitudinem vero figurae pro numero terminorum substituo, quoniam cum numerus terminorum supponatur* ∞ *erit omnium longitudinum aggregatum* $\frac{\infty}{2}$ *Basis* (*quia Basis jam est extremorum aggregatum*). *Cum autem cujuslibet* (*lineae vel parallelogrammi inscripti vel adscripti*) *crassities sive Altitudo supponatur* $\frac{1}{\infty}$ *Altitudinis figurae, in illam summa longitudinum* $\left(\frac{\infty}{2}\ Basis\right)$ *ducenda est; adeoque* $\frac{1}{\infty}$ *Alt. in* $\frac{\infty}{2}$ *Bas. erit area. Est autem* $\frac{1}{\infty} A \times \frac{\infty}{2} B = \frac{1}{2}AB$. *Adeo ut A* (*figurae altitudo*) *non solum longitudinum numerum, sed eundem in communem omnium altitudinem ductum exhibeat, quae quidem communis altitudo tanto minor supponenda est, quanto termini seu longitudines sunt plures.*

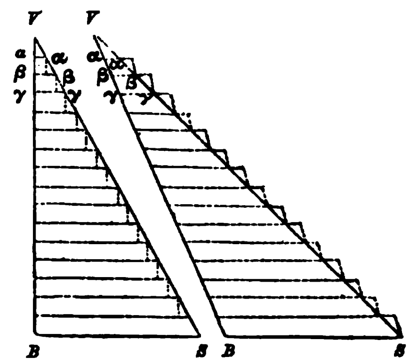
The demonstration in Wallis is therefore as we see of the following sort:

(1) In the progression of the natural numbers 1, 2, 3, 4, 5, etc., the sum is $\frac{(1+5)\,5}{2}$; because, as we should now say, in all arithmetical series two terms at an equal distance from the beginning and the end respectively will always, when added together, produce an equal sum; inasmuch as such a series can be annexed to itself in an inverted order, as

$$\begin{array}{c} 1,\ 2,\ 3,\ 4,\ 5 \\ \underline{5,\ 4,\ 3,\ 2,\ 1} \\ 6,\ 6,\ 6,\ 6,\ 6 \end{array}$$

whence, it is evident, that the sum of the series must be $\frac{(1+5)\,5}{2}$, or in general terms $\frac{(a+t)\,n}{2}$.

(2) In the triangle, if it be conceived as consisting of lines or small parallelograms, which we can suppose to be inscribed in or circumscribed about the triangle, these series of parallelograms, when compared with one another according to their relative magnitude, must form an arithmetical progression, of which the first term will be O, and the last term the base of the triangle. But the height of the triangle is the number of terms multiplied by the small altitude (or breadth), which is given to the lines or small parallelograms.



(3) The triangle being the sum of all these small parallelograms, we require only the arithmetical summation of the numerical progression from zero up to that term, which measures the fundamental line (or Base), and we are enabled to satisfy the formula $s=\frac{(a+t)\,n}{2}$, even when the number of terms is infinite, and when we have therefore to all appearance, neither t nor n given, (neither the last term nor the number of terms)—because for this purpose we pass from the domain of arithmetic into that of geometry, and we find that ($a=$ zero) and t, the longest of the parallel lines, or the longest of the many small parallelograms = the Base; and that thus, since $a=$ zero, $a+t$, the first and last term together = Base; which base we now therefore measure—and because the altitude of the triangle is equal to the thickness, which we assign to each line or parallelogram, multiplied into the supposed number of the said lines, therefore however thin and how many soever they may be, the altitude of the triangle is always $=\frac{1}{m}$ altitude $\times\, m$; that is to say, the seemingly indefinite number of the parallelograms, (the altitude whereof is $\frac{1}{m}$ of the altitude of the triangle) is the altitude of the

triangle, which therefore takes the place of the factor n. The formula of summation $s=\frac{a+t}{2}n$ is therefore satisfied, when the base is formally substituted for the t and the altitude for the n. Geometry and Arithmetic are thus reconciled with one another. Arithmetic says that the sum of every arithmetical progression $=\frac{a+t}{2}n$; and Geometry shows that this progression occurs in all triangles, when considered as composed of lines; besides which Geometry tells me, that the number of these terms (of which arithmetically I only know that it is infinite) comes to the same as the altitude or some other measurable part of the figure that lies before me, because each of the infinitesimal terms is an infinitesimal part of the altitude. It is equally manifest that the last term of this infinite series is the base of the figure, and the first term zero.

We now give briefly the leading propositions of the application, which Wallis proceeds to make of this idea; we must remark however that he has another demonstration of the rule $s=\frac{a+t}{2}n$ which he expresses as follows:

Si sumatur series quantitatum Arithmetice proportionalium, continue crescentium, a puncto vel 0 *inchoatarum et numero quidem vel finitarum vel infinitarum (nulla enim discriminis causa est) erit illa ad seriem totidem maximae aequalium ut* 1 *ad* 2.

Simplicissimus investigandi modus in hoc et sequentibus aliquot Problematis est rem ipsam aliquousque praestare et rationes prodeuntes observare, ut inductione tandem universalis propositio innotescat.

Est igitur exempli gratia:

$$\frac{0+1}{1+1}=\tfrac{1}{2}$$

$$\frac{0+1+2}{2+2+2}=\tfrac{3}{6}=\tfrac{1}{2}$$

$$\frac{0+1+2+3+4+5}{5+5+5+5+5+5}=\tfrac{15}{30}=\tfrac{1}{2}.$$

Et pari modo, quantumlibet progrediamur, prodibit semper ratio subdupla (1 *ad* 2).

It is therefore to the inductive method that Wallis gives the preference; and inasmuch as the first term is zero, the expression $\frac{a+t}{2}n$ reduces itself to $\frac{tn}{2}$; that is, the sum of the series bears to that of a like number of terms, each of which is as great as the greatest term of the series, [or to nt,] the proportion of 1 to 2, (*erit series ad seriem totidem maximæ æqualium, ut* 1 *ad* 2.

In like manner Wallis proves the further arithmetical propositions, for instance:

Prop. XIX. Si proponatur series Quantitatum in duplicata ratione Arithmetice proportionalium continue crescentium a puncto vel 0 *inchoatarum* (*puta ut* 0, 1, 4, 9 *etc.*) *propositum sit inquirere, quam habeat illa rationem ad seriem totidem maximae aequalium?*

Fiat investigatio per modum inductionis (*ut in prop.* 1) *eritque*

$$\frac{0+1=1}{1+1=2}=\tfrac{3}{6}=\tfrac{1}{3}+\tfrac{1}{6}$$

$$\frac{0+1+4=\ 5}{4+4+4=12}=\tfrac{1}{3}+\tfrac{1}{12}$$

$$\frac{0+1+4+9=14}{9+9+9+9=36}=\tfrac{1}{3}+\tfrac{1}{18}$$

$$\frac{0+\ 1+\ 4+\ 9+16=30}{16+16+16+16+16=80}=\tfrac{1}{3}+\tfrac{1}{24}.$$

Et sic deinceps. Ratio proveniens est ubique major quam subtripla seu $\frac{1}{3}$. *Excessus autem perpetuo decrescit prout numerus terminorum augetur.*

Adeoque (*Propos. XXI.*) *si proponatur series infinita, erit illa ad seriem totidem maximae aequalium ut* 1 *ad* 3.

Wallis had in the meantime remarked, in general terms, how great must be the continually decreasing excess, viz., *erit* (*posito multitudine terminorum m, et ultimi latere l*) $\frac{m}{3}.l^2+\frac{m}{6m-6}.l^2$; the last part of which

expression $\frac{m}{6m-6}\, l^2$ is the *excessus, qui perpetuò decrescit, prout numerus terminorum augetur.*

Thus Wallis had obtained the two important propositions—first, that the arithmetical progression, taken with a finite or infinite number of terms, bears to the sum of as many terms, all of the magnitude of the last term, the proportion of 1 to 2.

Secondly, that the infinite number of terms, if the series be composed of the second powers of an arithmetical progression, bears to the corresponding multiple of the last term the proportion of 1 to 3, which is here not exactly the case for a finite number of terms, but with an excess that is continually decreasing.

From these Wallis draws a number of corollaries, all which we here pass over; then he proves Prop. XXXIX., XL. and XLI., that if the series be conceived *in triplicatâ ratione arithmetice proportionalium continue crescentium*, as for instance 0, 1, 8, 27, 64, etc., their sum will be somewhat greater than $\frac{1}{4}$ that of as many numbers, each of which is assumed to be as great as the last term, but exactly $\frac{1}{4}$ thereof if they are taken in an infinite number.

Propositions XLII. and XLIII. next prove, that if a series is composed of fourth powers, their sum comes out in a similar manner $= \frac{1}{5}$, and in the fifth power $= \frac{1}{6}$, etc., all which Wallis comprehends in the proposition: *erit totius seriei ratio ad seriem totidem maximae aequalium, ut in hac tabella*

Primanorum	$\frac{1}{2}$	*id est*	1 *ad* 2
Secundanorum	$\frac{1}{3}$	. .	1 *ad* 3
Tertianorum	$\frac{1}{4}$	. .	1 *ad* 4
Quartanorum	$\frac{1}{5}$	. .	1 *ad* 5.

Et sic deinceps. Wallis therefore calls the simple *arithmetical* progressions *primana*, the second powers *secundana*, and so on.

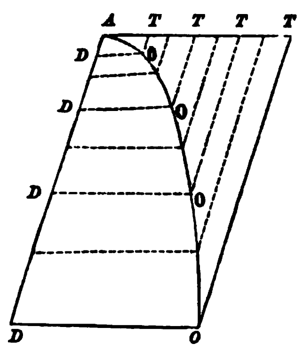
From the above follow then, in considering the law for these sums of series, the sums of the series of square roots, cube roots, etc., and thereafter Theorema LIV: *Si intelligitur series infinita quantitatum a*

puncto inchoatarum et continue crescentium pro ratione radicum quadraticarum, cubicarum, biquadraticarum etc. numerorum Arithmetice proportionalium (quam appello seriem Subsecundanorum, Subtertianorum, Subquartanorum etc.) erit totius ratio ad seriem totidem maximae aequalium ea quae sequitur in hac tabella, nempe:

Subsecundanorum	$\frac{2}{3}$ *id est ut*	1 *ad* $1\frac{1}{2}$
Subtertianorum	$\frac{3}{4}$	1 *ad* $1\frac{1}{3}$
Subquartanorum	$\frac{4}{5}$	1 *ad* $1\frac{1}{4}$
Subquintanorum	$\frac{5}{6}$	1 *ad* $1\frac{1}{5}$.

Et sic deinceps.

Without entering here also into the applications, which Wallis everywhere appends as corollaries to his theorems, we will here annex the one directly following Corollary. Prop. LV. Wallis says, *Ergo planum semiparabolae est vel etiam Parabolae ad Parallelogrammum circumscriptum ut* 2 *ad* 3. *Est enim Planum Semiparabolae (aut etiam Parabolae) series infinita subsecundanorum (per* 8 *prop. Con. Sect.); Parallelogrammum autem series totidem maximo aequalium. Ergo illud ad hoc ut* 1 *ad* $1\frac{1}{2}$ *vel ut* 2 *ad* 3.



The eighth proposition of the tract *De conicis sectionibus*, to which Wallis here refers us, as he does throughout this treatise, exhibited in a purely geometrical form the property of the parabola, that in it the *Quadrata applicatarum sunt interceptis Diametris proportionalia*, and that therefore the abscissae AD are to one another as the squares of the applicates [ordinates] $(DO)^2$; so that consequently if the applicates stand at an equal distance from one another, their squares, when imagined in a series, form an arithmetical progression, or the applicates themselves a progression of the roots of such a series. Therefore Wallis just says here briefly, that an infinite series *subsecundanorum* $\sqrt{x}$, (for instance the series $\sqrt{0}, \sqrt{1}, \sqrt{2}, \sqrt{3}, \sqrt{4}, \sqrt{5}$......continued *ad infinitum*)

bears to the rectangle (namely, to the infinite series of lines of equal magnitude, all as great as the last line of the parabola,) the proportion of 2 to 3.

After the several series of squares, cubes, &c., and also the series of square-roots, cube-roots and so on, have been summed, (and when thus the quadrature has been effected of all figures whose equations are $y = ax^n$ or $y = a\sqrt[n]{x}$,) the 58th proposition remarks concisely, that from the above follows the rule for the series composed of powers and roots; for since, e.g. *series subtertianorum* (*puta* $\sqrt[3]{0}, \sqrt[3]{1}, \sqrt[3]{2}, \sqrt[3]{3}, \sqrt[3]{4}$, *etc.*) *rationem habeant* (*ad seriem totidem maximæ æqualium*) *eam quæ est* 3 *ad* 4, *eorum quadrata* (*quæ eadem sunt et radices cubicæ serici secundanorum, puta* $\sqrt[3]{0}, \sqrt[3]{1}, \sqrt[3]{4}, \sqrt[3]{9}, \sqrt[3]{16}$, *etc.*) *rationem habebunt ad totidem maximæ æqualia, eam quæ est* 3 *ad* 5. *Quia nempe* $\frac{3}{3}$, $\frac{4}{3}$, $\frac{5}{3}$ *sunt arithmetice proportionalia.* It is now obvious, that all curves, or other regular geometric functions, whose equations are $y = ax^{\frac{m}{n}}$, are squared by this rule; and this is all comprehended* in Prop. LXIV., to which Wallis refers us in his preface, in order to call attention to its importance.

It will be remembered that in the Preface (after reference made to this most general proposition) we are told, *Deinde transii ad series auctas* (*quas voco,*) *et mutilatas sive deminutas, quæ ex duarum pluriumve serierum aggregatis constant.*

Of this we will give one example, which will make it readily intelligible.

Sit verbi gratia (in Prop. CVIII.) *terminus aequalium* (*et primanorum maximus*) R; *ejusque pars infinite parva dicatur* $a = \frac{R}{\infty}$; *numerus*

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* Wallis treats also of the cases with negative exponents, in which occur the several series $\frac{1}{1}, \frac{1}{2}, \frac{1}{3}, \frac{1}{4}$, *etc.*, $\frac{1}{\sqrt{1}}, \frac{1}{\sqrt{2}}, \frac{1}{\sqrt{3}}, \frac{1}{\sqrt{4}}$, *etc.*, and consequently the fractional equation $y = \frac{1}{x^m}$.

terminorum omnium (*vel figurae altitudo*) A.

	$R-0a$	$R+0a$
	$R-1a$	$R+1a$
	$R-2a$	$R+2a$
	$R-3a$	$R+3a$
	etc.	*etc.*
	etc.	*etc.*
si termini continuentur		
in infinitum usque ad	$R-R$	$R+R$
erit (*Residuorum*) *summa*..	$AR-\frac{1}{2}AR.$	
et (*Aggregatorum*) *summa*	....	$AR+\frac{1}{2}AR.$

This is repeated in a still more general form in Prop. CXI.,

Nempe si termini	$R^2\mp 0a^2$	$R^3\mp 0a^3$	$R^4\mp 0a^4$
	$R^2\mp 1a^2$	$R^3\mp 1a^3$	$R^4\mp 1a^4$
	$R^2\mp 2a^2$	$R^3\mp 2a^3$	$R^4\mp 2a^4$
	$R^2\mp 3a^2$	$R^3\mp 3a^3$	$R^4\mp 3a^4$
	etc.	*etc.*	*etc.*
	etc.	*etc.*	*etc.*
continuentur usque ad	$R^2\mp R^2$	$R^3\mp R^3$	$R^4\mp R^4$
Summae	$AR^2\mp\frac{1}{3}AR^2$	$AR^3\mp\frac{1}{4}AR^3$	$AR^4\mp\frac{1}{5}AR^4$
Hoc est residuorum summa	$\frac{2}{3}$	$\frac{3}{4}$	$\frac{4}{5}$
et Aggregatorum summa	$\frac{4}{3}.$	$\frac{5}{4}$	$\frac{6}{5}$

It is evident that Wallis had discovered Integration in the easier cases. The geometrical idea of Cavalleri and Gregoire de St. Vincent, of conceiving a surface as composed of lines, was by Wallis converted into an arithmetical one, and the process of summation was thus brought into the foreground. Even now a learner, who never readily comprehends what integration as a method of effecting the quadrature of geometrical surfaces really is, cannot have a more distinct idea of it given him than by the plan and teaching of Wallis, who, beginning from

the simplest case in the triangle, shows that the infinite number of small parallelograms, (or if you will, of lines) added together give the height of the triangle, and that consequently the formula of summation $s=\frac{(a+t)n}{2}$, in the case in which the number of terms is infinite, is applicable to geometry, and that so are likewise other arithmetical formulas of summation in other figures.

CHAPTER III.

THE INVENTION OF THE DIFFERENTIAL CALCULUS.

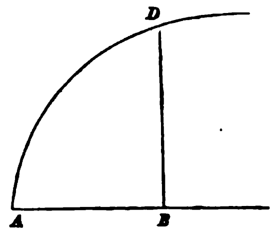
We have seen that Barrow appended an arithmetical method of tangents to his geometrical works, and that for geometrical quadratures Wallis taught in general the arithmetical process of summation. When Mercator had discovered the quadrature of the hyperbola, of which Wallis had said *hic hæret aqua*, and when this created great sensation, Barrow wrote back;* *A friend of mine brought me the other day some papers, wherein he hath set down methods of calculating the Dimensions of Magnitudes like that of Mr. Mercator for the Hyperbola, but very general;* and a couple of days later, *I am glad my Friend's Paper gives you so much satisfaction; his name is Newton, a Fellow of our College, and very young, but of an extraordinary Genius and Proficiency in these things.*

These papers were headed, *De analysi per æquationes numero terminorum infinitas*, and begin with the words *Methodum generalem, quam*

* *Amicus quidam*, according to the Latin version in the *Commercium Epistolicum*, *nudius tertius chartas quasdam mihi tradidit, in quibus magnitudinum dimensiones supputandi Methodos Mercatoris methodo pro Hyperbolâ similes, maxime vero generales descripsit......est illi nomen Newtonus, juvenis, et qui eximio quo est acumine magnos in hac re progressus fecit.* This is the passage that Weissenborn did not cite, but we suppose had in view when he assumed (what in the meantime is however doubtful) that Newton was also that *amicus* who (just at this time, for Barrow's work was published 1670) pressed Barrow, as Barrow said, to append to his geometrical work the arithmetical process for calculating tangents (see above, page 4).

de Curvarum quantitate per infinitam terminorum Seriem mensuranda olim excogitaveram, in sequentibus breviter explicatam potius quàm accurate demonstratam habes.

The contents of this compendium are, in the brief words of Newton, (leaving out all the calculations and applications) as follows: *Basi AB (Curvae alicuius AD) sit applicata BD perpendicularis: Et vocetur AB=x, BD=y et sint a, b, c etc. Quantitates datae et m, n, Numeri integri. Deinde*



CURVARUM SIMPLICIUM QUADRATURA.

REGULA I. *Si* $ax^{\frac{m}{n}} = y$; *erit* $\frac{an}{m+n} x^{\frac{m+n}{n}} = $ *Area ABD.*

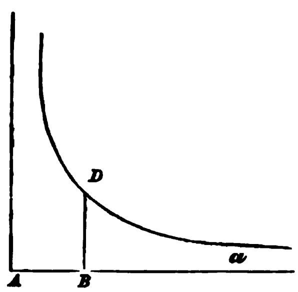
RES EXEMPLO PATEBIT:

(1) *Si* $x^2 = 1x^{\frac{2}{1}} = y$; *erit* $\frac{1}{3}x^3 = ABD$.

(4) *Si* $\frac{1}{x^2} = x^{-2} = y$;

erit $= \frac{1}{-1} x^{-\frac{1}{1}} = -x^{-1} = -\frac{1}{x} = aBD$,

infinite versus a protensae; quam calculus ponit negativam, propterea quod jacet ex altera parte lineae BD.



COMPOSITARUM CURVARUM QUADRATURA EX SIMPLICIBUS.

REGULA II. *Si valor ipsius y ex pluribus istiusmodi Terminis componitur, Area etiam componetur ex Areis quae a singulis Terminis emanant.*

EXEMPLA:

Si $x^2 + x^{\frac{3}{2}} = y$; *erit* $\frac{1}{3}x^3 + \frac{2}{5}x^{\frac{5}{2}} = ABD$. *Et si* $3x - 2x^2 + x^3 - 5x^4 = y$; *Erit* $\frac{3}{2}x^2 - \frac{2}{3}x^3 + \frac{1}{4}x^4 - x^5 = ABD$.

ALIORUM OMNIUM QUADRATURA.

REGULA III. *Si valor ipsius y vel aliquis ejus Terminus sit praecedentibus magis compositus, in Terminos Simpliciores reducendus est, operando in literis ad eundem Modum, quo Arithmetici in numeris Decimalibus dividunt, Radices extrahunt, vel affectas aequationes solvunt; et ex istis Terminis quaesitam Curvae superficiem, per praecedentes Regulas deinceps elicies.*

EXEMPLA DIVIDENDO.

Sit $\frac{a^2}{b+x}=y$; *Curva nempe existente Hyperbola—Divisionem instituo—et sic vice hujus* $y=\frac{a^2}{b+x}$ *prodit* $y=\frac{a^2}{b}-\frac{a^2x}{b^2}+\frac{a^2x^2}{b^3}-\frac{a^2x^3}{b^4}$ *etc. serie ista infinite continuata; Adeoque* (*per Regulam Secundam*) *Area quaesita* $ABDC$ *erit* $=\frac{a^2x}{b}-\frac{a^2x^2}{2b^2}+\frac{a^2x^3}{3b^3}-\frac{a^2x^4}{4b^4}$ *etc. infinitae etiam seriei, cuius tamen pauci initiales sunt in usum quemvis satis exacti, si modo* x *sit aliquoties minor quam* b *etc.* (six pages).

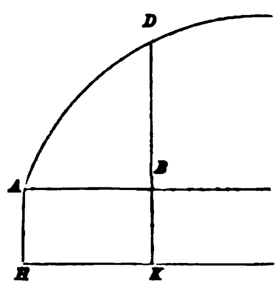
Et haec de areis curvarum investigandis dicta sufficiant.

Imo cum Problemata omnia de curvarum Longitudine de quantitate et superficie Solidorum, deque Centro Gravitatis, possunt eo tandem reduci ut quaeratur quantitas Superficiei planae linea curva terminatae, non opus est quicquam de iis adjungere.

In istis autem quo ego operor modo dicam brevissime.

APPLICATIO PRAEDICTORUM AD RELIQUA ISTIUSMODI PROBLEMATA.

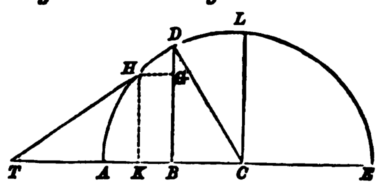
Sit ABD *curva quaevis, et* $AHKB$ *rectangulum cuius latus* AH *vel* BK *est unitas. Et cogita rectam* DBK *uniformiter ab* AH *motam, areas* ABD *et* AK *describere; et quod* BK (1) *sit momentum quo* $AK(x)$ *et* $BD(y)$ *momentum quo* ABD *gradatim augetur; et quod ex momento* BD *perpetim dato, possis, per praedictas regulas, aream* ABD *ipso descriptam investigare, sive cum area* $AK(x)$ *momento* 1 *descripta conferre.*



Jam qua ratione Superficies ABD ex momento suo perpetim dato, per praecedentes regulas elicitur, eadem quaelibet alia quantitas ex momento suo sic dato elicietur. Exemplo res fiet clarior.

LONGITUDINES CURVARUM INVENIRE.

Sit ADLE circulus cujus arcus AD longitudo est indaganda. Ducto tangente DHT, et completo indefinite parvo rectangulo HGBK, et posito $AE = 1 = 2AC$. *Erit ut BK sive GH, momentum Basis* $AB(x)$, *ad HD momentum Arcus* $AD :: BT : DT :: BD$ $(\sqrt{x-xx}) : DC(\frac{1}{2}) :: 1(BK) : \frac{1}{2\sqrt{x-xx}}(DH)$. *Adeoque* $\frac{1}{2\sqrt{x-xx}}$ *sive* $\frac{2\sqrt{x-2xx}}{2x-2xx}$ *est momentum Arcus AD. Quod reductum fit*



$$\tfrac{1}{2}x^{-\frac{1}{2}} + \tfrac{1}{4}x^{\frac{1}{2}} + \tfrac{3}{16}x^{\frac{3}{2}} + \tfrac{1}{32}x^{\frac{5}{2}} + \tfrac{1}{256}x^{\frac{7}{2}} + \tfrac{63}{512}x^{\frac{9}{2}}, \text{ etc.}$$

Quare per regulam secundam, longitudo Arcus AD est

$$x^{\frac{1}{2}} + \tfrac{1}{6}x^{\frac{3}{2}} + \tfrac{3}{40}x^{\frac{5}{2}} + \tfrac{5}{112}x^{\frac{7}{2}} + \tfrac{35}{1152}x^{\frac{9}{2}} + \tfrac{63}{2816}x^{\frac{11}{2}} \text{ etc.}$$

$$\textit{sive } x^{\frac{1}{2}} \textit{ in } 1 + \tfrac{1}{6}x + \tfrac{3}{40}x^2 + \tfrac{5}{112}x^3 + \tfrac{35}{1152}x^4 + \tfrac{63}{2816}x^5, \text{ etc.}$$

Non secus ponendo CB esse x, et radium CA esse 1, *invenies Arcum LD esse* $x + \frac{1}{6}x^3 + \frac{3}{40}x^5 + \frac{5}{112}x^7$, *etc.*

Sed notandum est quod unitas ista quae pro momento ponitur, est Superficies cum de Solidis, et linea cum de superficiebus, et punctum cum de lineis (ut in hoc exemplo) agitur.

Nec vereor loqui de unitate in punctis, sive lineis infinite parvis, si quidem proportiones ibi jam contemplantur Geometrae, dum utuntur methodis Indivisibilium.

Ex his fiat conjectura de superficiebus et quantitatibus solidorum, ac de Centris Gravitatum.

Under the headings—

Invenire prædictorum conversum,

and

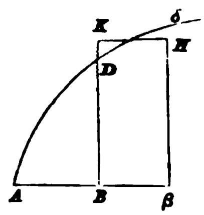
Inventio Basis ex Area data,

together with

Inventio Basis ex Longitudine Curvæ,

Newton proceeds to give that which is announced under these titles, and closes the compendium with the demonstration of the two leading propositions. *Respicienti,* says he, *duo præ reliquis demonstranda occurrunt.*

I. DEMONSTRATIO QUADRATURAE CURVARUM SIMPLICIUM IN REGULA PRIMA. PRAEPARATIO PRO REGULA PRIMA DEMONSTRANDA.



Sit itaque curvae alicujus $AD\delta$ Basis $AB = x$, perpendiculariter applicata $BD = y$, et area $ABD = z$, ut prius. Item sit $B\beta = o$, $BK = v$, et rectangulum $B\beta HK\,(ov)$ aequale spatio $B\beta\delta D$.

Est ergo $A\beta = x + o$, et $A\delta\beta = z + ov$. His praemissis, ex relatione inter x et z ad arbitrium assumpta quaero y isto, quem sequentem vides, modo.

*Pro lubitu sumatur $\frac{2}{3}x^{\frac{3}{2}} = z$, sive $\frac{4}{9}x^3 = zz$. Tum $x + o\,(A\beta)$ pro x, et $z + ov\,(A\delta\beta)$ pro z substitutis, prodibit $\frac{4}{9}$ in $x^3 + 3x^2o + 3xo^2 + o^3 =$ (ex natura Curvae) $z^2 + 2zov + o^2v^2$. Et sublatis ($\frac{4}{9}x^3$ et zz) aequalibus, reliquisque per o divisis, restat $\frac{4}{9}$ in $3x^2 + 3xo + o^2 = 2zv + ov^2$. Si jam supponamus $B\beta$ in infinitum diminui et evanescere, sive o esse nihil, erunt v et y aequales, et termini per o multiplicati evanescent, quare restabit $\frac{4}{9} \times 3xx = 2zv$, sive $\frac{2}{3}xx\,(= zy) = \frac{2}{3}yx^{\frac{3}{2}}$, sive $x^{\frac{1}{2}}\left(= \frac{x^2}{x^{\frac{3}{2}}}\right) = y$. Quare e contra si $x^{\frac{1}{2}} = y$, erit $\frac{2}{3}x^{\frac{3}{2}}y = z$.**

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* Newton chooses here a complicated case; but if we take the most simple example, which he gives somewhere $z = x^3$; then, (if we suppose again that, in order to retain the figure, $x = AB$; $x + o = AB$; $z = ADB$) we have

$$(x + o)^3 = x^3 + 3ox^2 + 3o^2x + o^3 = z + ov;$$

consequently $3ox^2 + 3o^2x + o^3 = ov$; therefore $3x^2 + 3ox + o^2 = v$. Now $o =$ zerò gives $3x^2 = v = y$.

DEMONSTRATIO.

Vel generaliter, si $\frac{n}{m+n} \times ax^{\frac{m+n}{n}} = z$; *sive, ponendo* $\frac{na}{m+n} = c$, *et* $m+n=p$, *si* $cx^{\frac{p}{n}} = z$ *sive* $c^n x^p = z^n$: *tum* $x+o$ *pro* x, *et* $z+ov$ (*sive, quod perinde est,* $z+oy$) *pro* z, *substitutis, prodit* c^n *in* $x^p + pox^{p-1}$, *etc.* $= z^n + noyz^{n-1}$; *etc. reliquis nempe terminis, qui tandem evanescerent, omissis. Jam sublatis* $c^n x^p$ *et* z^n *aequalibus, reliquisque per* o *divisis, restat* $c^n px^{p-1} = nyz^{n-1}$ $\left(= \frac{nyz^n}{z} = \frac{nyc^n x^p}{cx^{\frac{p}{n}}}\right)$ *sive dividendo per* $c^n x^p$, *erit* $px^{-1} = \frac{ny}{cx^{\frac{p}{n}}}$ *sive* $pcx^{\frac{p-n}{n}} = ny$; *vel restituendo* $\frac{na}{m+n}$ *pro* c, *et* $m+n$ *pro* p, *hoc est,* m *pro* $p-n$, *et* na *pro* pc, *fiet* $ax^{\frac{m}{n}} = y$. *Quare e contra, si* $ax^{\frac{m}{n}} = y$, *erit* $\frac{n}{m+n} ax^{\frac{m+n}{n}} = z$. Q.E.D.

This is therefore Newton's compendium of the Differential Calculus, which in 1669 was sent from Cambridge to the President of the Society in London, as likewise to Collins. To this Newton added his Tangential Method in the letter which he wrote to Collins on the 10th of December, 1672, in which he said, as the reader will recollect, that he was glad that Barrow's "Lectiones" had met with so much approbation, and that his (Newton's) Method of Tangents which was indicated in the letter by an example, belonged to his universal method of Quadratures.

Respecting the sensation which this discovery gave rise to, see the letters (printed in the *Comm. Epist.*) in which it was announced in Italy, France, and Holland. When Leibnitz had heard of it, and, speaking of some results of it which had come to his notice, wrote, *rem gratam feceris si demonstrationem transmiseris* (letter to Oldenburg, of 22nd May, 1676), Newton wrote concerning his method and his invention, not yet published in detail, the letters to Leibnitz, which are printed in the *Commercium Epistolicum*. Newton begins by

saying, *Quamquam Dom. Leibnitii modestia in excerptis quæ ex epistolâ ejus ad me nuper misisti, nostratibus multum tribuit circa Speculationem quandam infinitarum serierum de quâ jam cœpit esse rumor...... quoniam tamen ea scire pervelit quæ ab Anglis hâc in re inventa sunt*,—he is therefore willing to communicate some information. With regard to infinite series, the idea, he intimates, through which they were at first discovered by him, was that of extracting from algebraical expressions, in the same manner as from decimal fractions, the roots which could not be otherwise obtained. But inasmuch as all expressions of roots and divisions could be regarded as powers with fractional or negative exponents; since, instead of $\sqrt{a}$, $\sqrt{a^3}$, etc., we could write the forms $a^{\frac{1}{2}}$, $a^{\frac{3}{2}}$, and instead of $\frac{1}{a^3}$, $\frac{1}{\sqrt[4]{a^5}}$, the forms a^{-3}, $a^{-\frac{5}{4}}$, and so, in complicated cases, instead of $\frac{a^2}{\sqrt[4]{a^3+bx^2}}$ the form $a^2(a^3+bx^2)^{-\frac{1}{4}}$; therefore all the rules for the extraction of roots, were comprised in the one theorem which (he says) he will impart to Leibnitz, viz.*

$$(P+PQ)^{\frac{m}{n}} = P^{\frac{m}{n}} + \frac{m}{n} AQ + \frac{m-n}{2n} BQ + \frac{m-2n}{4n} CQ \text{ etc.}$$

The generality, continued Newton, of his method could be easily shown by examples; since,† for instance, the expression $\sqrt{c^2+x^2}$ was by this method changed into the series $c + \left(\frac{1}{2c}\right)x^2 - \left(\frac{1}{8c^3}\right)x^4 + \left(\frac{1}{16c^5}\right)x^6 - \&c.$ As a further example Newton chooses the expression $\sqrt[5]{c^5+c^4x-x^5}$,

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* By this means every curve, however complicated might be its equation, provided it was not an implicit one, was brought under Wallis's rules of Quadrature (summations) which in the letter were presumed to be known and were known to Leibnitz, as to all geometers, since 1659.

† This refers, as Leibnitz knew, to the expression for the hyperbola, of which the quadrature (just given by Mercator) had appeared so difficult, and of which Wallis had said hic haeret aqua. Wallis' rule of summation is applicable to the series and not to the finite expression $\sqrt{c^2+x^2}$.

or $(c^5 + c^4x - x^5)^{\frac{1}{5}}$. Of fractional expressions also Newton annexes a few examples, viz. the cases in which there might come into the equation of the curve terms such as $\frac{1}{a+x}$ or $\frac{1}{(a+x)^2}$, or $\frac{a}{\sqrt[5]{b^3+3b^2x+3bx^2+x^3}}$.

But if the equation were yet more complicated, that is to say, "affected,"* then the roots could be sought by approximation, of which Newton gives two examples.

He then continues, *Quomodo ex æquationibus sic ad infinitas series reductis areæ et longitudines curvarum contenta et superficies solidorum vel quorum libet segmentorum figurarum quarumvis eorumque centra gravitatis determinentur .... nimis longum foret describere; sufficiat specimina quædam talium problematum recensuisse, inque iis brevitatis gratiâ literas A, B, C etc., pro terminis seriei sicut sub initio nonnunquam usurpabo.* With this introduction Newton gives, in nine examples, the entire results of his Analysis.†

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* This expression refers to what is now called an implicit equation. Thus Newton having heretofore brought under the Wallisian theorems, or (as we may say) method of Integration and rule of Quadrature, all curves with equations such as $y = \sqrt{c^2+x^2}$; $y = \sqrt[5]{c^5 + c^4x - x^5}$, etc., or in general $y = f(x)$, turns his attention here at last to the implicit functions, $f(x, y) = 0$, *i.e.* to expressions such as

$$y^3 + axy + x^2y - x^3 - a^3 = 0.$$

† The difference between the contents of the Analysis, and that which is here communicated to Leibnitz, will be rendered evident by the following example. In the Analysis we read, (compare above, page 23) *Longitudines curvarum invenire. Sit ADLE circulus* (where $AB = x$, therefore since $AE = 1$, $(DB)^2 = x(1-x)$, or $DB = \sqrt{x(1-x)} = \sqrt{x-x^2}$) *cujus arcus AD longitudo est indaganda. Ducto tangente DHT et completo indefinite parvo rectangulo HGBK, et posito $AE = 1 = 2AC$; Erit ut BK sive GH, momentum Basis AB* (x) *ad HD momentum Arcus AD*

$$:: BT : DT = BD \ (sive\ \sqrt{x-x^2}) : DC \ (sive\ \tfrac{1}{2}) = 1\ (BK) : \frac{1}{2\sqrt{x-x^2}}\ (DH)\ Adeo\text{-}$$

After Leibnitz had answered this letter, and requested further explanations, Newton says in his letter of 24th October, 1676, that the way in which he had hit upon a part of his method in the commencement of his studies was this, that he had endeavoured to interpolate the series, the interpolation of which Wallis had declared to be necessary for Quadratures that were too difficult for him.* He had written [he says] a compendium of his whole method, which had been communicated through Barrow to Collins, *in quo significaveram Areas et Longitudines Curvarum omnium et Solidorum Superficies et Contenta, ex datis Rectis et vice versâ ex his datis Rectas determinari posse.* When he afterwards wished to make a treatise out of this, he had added [he states] other things, and in particular the method of drawing Tangents, which Sluse also had discovered, (but with this difference, that the method of Newton was applicable to complicated curves), but this method of Tangents and other things he preferred [he says] not to communicate to Leibnitz.

To this Leibnitz answers in these words, in which it is said is contained his independent discovery of the Differential Calculus, viz. that

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que $\frac{1}{2\sqrt{x-x^2}}$ *sive* $\frac{\sqrt{x-x^2}}{2x-2x^2}$ *est momentum arcus AD. Quod reductum fit*

$$\tfrac{1}{2}x^{-\frac{1}{2}} + \tfrac{1}{4}x^{\frac{1}{2}} + \tfrac{3}{16}x^{\frac{3}{2}} + \tfrac{5}{32}x^{\frac{5}{2}} + \tfrac{35}{256}x^{\frac{7}{2}} \textit{ etc.}$$

Quare per regulam secundam (one of the Wallisian rules of Quadrature or Summation, which Newton makes use of) *longitudo arcus AD est*

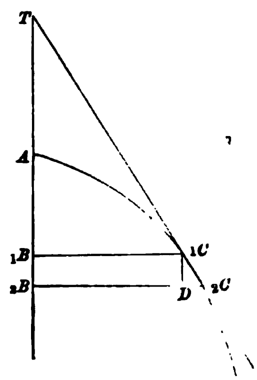
$$x^{\frac{1}{2}} + \tfrac{1}{6}x^{\frac{3}{2}} + \tfrac{3}{40}x^{\frac{5}{2}} + \tfrac{5}{112}x^{\frac{7}{2}} + \tfrac{35}{1152}x^{\frac{9}{2}} \textit{ etc.}$$

Instead of these words and the accompanying figure in the Analysis, Newton in his letter to Leibnitz gives only the result: *Si ex dato sinu vel sinu verso arcus desideratur et d diameter ac x sinus versus, erit arcus*

$$= d^{\frac{1}{2}}x^{\frac{1}{2}} + \frac{x^{\frac{3}{2}}}{6d^{\frac{1}{2}}} + \frac{3x^{\frac{5}{2}}}{40d^{\frac{3}{2}}} \text{ (or } x^{\frac{1}{2}} + \tfrac{1}{6}x^{\frac{3}{2}} + \tfrac{3}{40}x^{\frac{5}{2}} \text{ if } d = 1).$$

* Newton's words are: *Sub initio studiorum meorum mathematicorum ubi incideram in opera Wallisii, considerando Series quarum intercalatione ipse circuli Aream etc.* This is the place where Wallis, in his preface, had said, *hic haeret aqua.*

whereas Sluse's method of tangents was not sufficient, he had discovered another, and to this he adds, *arbitror quæ celare voluit Newtonus de Tangentibus ducendis ab his non abludere.* Leibnitz's words are: *Clarissimi Slusii Methodum Tangentium nondum esse absolutam celeberrimo Newtono assentior. Et jam a multo tempore rem Tangentium longe generalius tractavi; scilicet per differentias Ordinatarum. Nempe* $T\ 1B$ *(intervallum Tangentis ab Ordinata in Axe sumptum) est ad* $1B\ 1C$ *Ordinatam, ut* $1CD$ *(differentia duarum Abscissarum* $A\ 1B$, $A\ 2B$*) ad* $D\ 2C$ *(differentiam duarum Ordinatarum* $1B\ 1C$, $2B\ 2C$*). Nec refert quem angulum faciunt Ordinatae ad Axem. Unde patet, nihil aliud esse invenire Tangentes, quam invenire Differentias Ordinatarum, positis differentiis Abscissarum (seu* $1B\ 2B = 1CD$*) si placet aequalibus. Hinc nominando (in posterum)* $\mathrm{d}y$ *differentiam duarum proximarum* y *(nempe* $A\ 1B$ *et* $A\ 2B$*); et* $\mathrm{d}x$ *seu* $D\ 2C$ *differentiam duarum proximarum* x *(prioris* $1B\ 1C$, *posterioris* $2B\ 2C$*); patet* $\mathrm{d}y^2$ *esse* $2y\,\mathrm{d}y$*; et* $\mathrm{d}y^3$ *esse* $3y^2\mathrm{d}y$, *etc. et ita porro. Nam sint duae proximae sibi (id est, differentiam habentes infinite parvam) scilicet* $A\ 1B = y$*; et* $A\ 2B = y + \mathrm{d}y$. *Quoniam ponimus* $\mathrm{d}y^2$ *esse differentiam quadratorum ab his duabus rectis, Aequatio erit* $\mathrm{d}y^2 = y^2 + 2y\,\mathrm{d}y + \mathrm{d}y\,\mathrm{d}y - y^2$. *Seu omissis* $y^2 - y^2$ *quae se destruunt, item omisso quadrato quantitatis infinite parvae (ob rationes ex Methodo de Maximis et Minimis notas), erit* $\mathrm{d}y^2 = 2y\,\mathrm{d}y$. *Idemqae est de caeteris potentiis. Hinc etiam haberi possunt differentiae quantitatum ex diversis indefinitis in se invicem ductis factarum: ut* $\mathrm{d}yx$ *erit* $= y\,\mathrm{d}x + x\,\mathrm{d}y$*; et* $\mathrm{d}y^2x = 2xy\,\mathrm{d}y + y^2\mathrm{d}x$. *Hinc si aequatio*



$$a + by + cx + dyx + ey^2 + fx^2 + gy^2x + hyx^2 \text{ etc.} = 0\,;$$

statim habetur Tangens Curvae ad quam est ista Aequatio. Nam ponendo $AB = y$, *et* $A\ 2B = y + \mathrm{d}y$ *(scilicet, quia* $1B\ 2B$ *seu* $1CD = \mathrm{d}y$*); Itemque ponendo* $1B\ 1C = x$, *et* $2B\ 2C = x + \mathrm{d}x$ *(scilicet, quia* $2CD = \mathrm{d}x$*), et quia*

eadem aequatio exprimit quoque relationem inter $A\ 2B$ *et* $2B\ 2C$, *quae eam exprimebat inter* $A\ 1B$ *et* $1B\ 1C$; *Tunc in aequatione illa pro* y *et* x *substituendo* $y + \mathrm{d}y$, *et* $x + \mathrm{d}x$, *fiet*

$$\left.\begin{array}{l}
a + by + cx + dyx + ey^2 + fx^2 + gy^2x + hyx^2 \text{ etc.} \\
\hline
b\mathrm{d}y + c\mathrm{d}x + dy\mathrm{d}x + 2ey\mathrm{d}y + 2fx\mathrm{d}x + 2gxy\mathrm{d}y + 2hxy\mathrm{d}x \text{ etc.} \\
\qquad + dx\mathrm{d}y \qquad\qquad\qquad\qquad + gy^2\mathrm{d}x + hx^2\mathrm{d}y \text{ etc.} \\
\hline
\qquad + d\mathrm{d}x\mathrm{d}y + e\mathrm{d}y\mathrm{d}y + f\mathrm{d}x\mathrm{d}x + gx\mathrm{d}y\mathrm{d}y + hy\mathrm{d}x\mathrm{d}x \\
d \text{ est quantitas communi more.} \qquad + 2gy\mathrm{d}y\mathrm{d}x + 2hx\mathrm{d}x\mathrm{d}y \text{ etc.} \\
\mathrm{d} \text{ est nota Differentiae.} \qquad + g\mathrm{d}x\mathrm{d}y\mathrm{d}y + h\mathrm{d}y\mathrm{d}x\mathrm{d}x
\end{array}\right\} = 0.$$

Ubi, abjectis illis quae sunt supra primam lineam, quippe nihilo aequalibus per aequationem praecedentem; et abjectis illis quae sunt infra secundam, quia in illis duae infinite parvae in se invicem ducuntur; hinc restabit tantum aequatio haec $b\mathrm{d}y + c\mathrm{d}x + dy\mathrm{d}x$
$+ dx\mathrm{d}y$ *etc.* $= 0$, *quicquid scilicet reperitur inter lineam primam et secundam. Et mutata aequatione in rationem seu Analogiam, fiet*

$$-\frac{\mathrm{d}y}{\mathrm{d}x} = \frac{c + dy + 2fx + gy^2 + 2hxy \text{ etc.}}{b + dx + 2ey + 2gxy + hx^2 \text{ etc.}}.$$

Id est

$$\left(\textit{quia} - \frac{\mathrm{d}y}{\mathrm{d}x} \textit{ seu } \frac{-1B\,2B, \textit{ seu} - 1\,CD}{D\,2C} = -\frac{T\,1B}{1B\,2C}\right) \textit{erit } \frac{c + dy \text{ etc.}}{b + dx \text{ etc.}} = -\frac{T\,1B}{1B\,1C}.$$

Quod coincidit cum Regula Slusiana, ostenditque eam statim occurrere hanc Methodum intelligenti.

Sed Methodus ipsa (*priore*) *nostra longe est amplior. Non tantum enim exhiberi potest, cum plures sunt literae indeterminatae quam* y *et* x (*quod saepe fit maximo cum fructu*); *Sed et tunc utilis est cum interveniunt irrationales, quippe quae eam nullo morantur modo, neque ullo modo necesse est irrationales tolli, quod in Methodo Slusii necesse est, et calculi difficultatem in immensum auget.*

Quod ut appareat, tantum utile erit in irrationalitatibus simplicioribus rem explanare. Et primum sit in simplicissimis generaliter. Si sit aliqua potentia aut radix x^z; *erit* $\mathrm{d}x^z = zx^{z-1}\,\mathrm{d}x$.

Si z sit $\frac{1}{2}$, seu si x^z sit $\sqrt{x}$, erit $\mathrm{d}x^z$, *seu hoc loco* $\mathrm{d}\sqrt{x} = \frac{1}{2}x^{-\frac{1}{2}}\,dx$ *seu* $\dfrac{\mathrm{d}x}{2\sqrt{x}}$; *ut notum aut facile demonstrabile.*

Sit jam Binomium, ut

$$\sqrt{}^{3} : \overline{a + by + cy^2 \textit{ etc.}} \textit{ quaeritur } \mathrm{d}\sqrt{}^{3} : \overline{a + by + cy^2 \textit{ etc.}} \textit{ seu } \mathrm{d}x^z,$$

posito $\frac{1}{3} = z$, *et* $a + by + cy^2$ *etc.* $= x$. *Est autem* $\mathrm{d}x = b\,\mathrm{d}y + 2cy\,\mathrm{d}y$ *etc. Ergo* $\mathrm{d}x^z$ *seu* $\dfrac{\mathrm{d}x}{3x^{\frac{2}{3}}} = \dfrac{b\,\mathrm{d}y + 2cy\,\mathrm{d}y \textit{ etc.}}{3 \times \overline{a + by + cy^2 \textit{ etc.}}\rceil^{\frac{2}{3}}}$. *Eadem Methodus adhiberi potest etsi Radices in Radicibus implicentur. Hinc si detur aequatio valde intricata, ut*

$$a + bx\sqrt{y^2 + b\sqrt{}^{3} : \overline{1+y}} + hx^2y\sqrt{y^2 + y\sqrt{1-y}} = o,$$

ad aliquam Curvam cuius Abscissa sit y (AB), Ordinata x (BC), tunc Aequatio proveniens utilis ad inveniendam Tangentem TC, statim sine calculo scribi poterit; et erit haec

$$b\,\mathrm{d}x\sqrt{y^2 + b\sqrt{}^{3} : \overline{1+y}} + \frac{bx}{2\sqrt{y^2 + b\sqrt{}^{3} : \overline{1+y}}} \times \overline{2y\,\mathrm{d}y + \frac{b\,\mathrm{d}y}{3 \times \overline{1+y}\rceil^{\frac{2}{3}}}}$$

$$+ \overline{hx^2\,\mathrm{d}y + 2hxy\,\mathrm{d}x} \times \sqrt{y^2 + y\sqrt{1-y}}$$

$$+ \frac{hyx^2}{2\sqrt{y^2 + y\sqrt{1-y}}} \times \overline{2y\,\mathrm{d}y + \mathrm{d}y\sqrt{1-y} - \frac{y\,\mathrm{d}y}{2\sqrt{1-y}}} = 0.$$

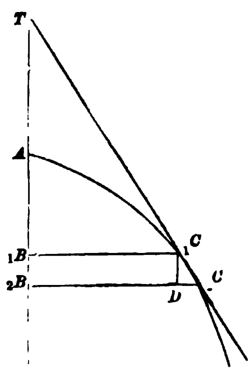
Seu, mutando Quotientem hanc inventam in Analogiam, erit—$\mathrm{d}y$ *ad* $\mathrm{d}x$, *seu $T\,1B$ ad $1B\,1C$, ut omnes provenientis aequationis termini per* $\mathrm{d}x$ *multiplicati, ad omnes ejusdem terminos per* $\mathrm{d}y$ *multiplicatos.*

Ubi sane mirum et maxime commodum evenit, quod $\mathrm{d}y$ *et* $\mathrm{d}x$ *semper extant extra vinculum irrationale. Methodo autem Slusiana omnes ordine irrationales tollendas esse nemo non videt.*

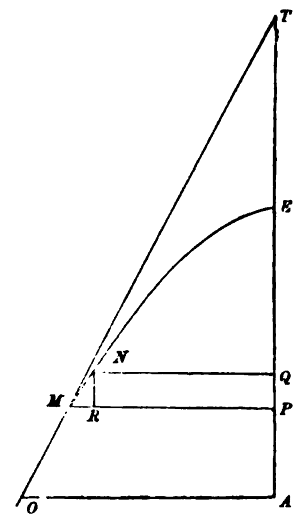
Arbitror, quae celare voluit Newtonus de Tangentibus ducendis, ab his non abludere.

We see here that Leibnitz avoids making any mention of a

Newtonian method of tangents. We remember Barrow's method of tangents in which he had given:



a figure and a process quite similar to those of Leibnitz's letter:



in which figure, (as may be seen above Chapter I., page 5) Barrow took $MR = a$, $NR = e$, and gave the rules: *Inter computandum omnes abjicio terminos, in quibus ipsarum a, vel e, potestas habetur vel in quibus ipsae ducuntur in se*, and we remember that Newton, supplementing Barrow, said in his letter of 10th December, 1672, that his (Newton's) method of Tangents could be apprehended from an example, in which he mentions the rule and then says: *hoc est unum particulare methodi generalis, quae extendit se non modo ad ducendum tangentes verum etiam ad resolvendum alia abstrusiora problemata de curvitatibus.* We repeat that, if Leibnitz was acquainted with this letter of Newton's, in which was contained the Newtonian method, which was Leibnitz's method, he ought not to have avoided the mention of this method, and ought not therefore to have written, *arbitror quæ celare voluit Newtonus de Tangentibus ducendis ab his non abludere*, but to have written: *quæ celare voluisset Newtonus mihi cognita sunt, nam literas ejus 10mi Decembris*, 1672, *inspexi.*

Nor let it be said, that it was Newton's business to remember in 1676 that he had in 1672 written that letter on Tangents to Collins, and that Collins might perhaps have communicated it to a friend; or that, if Newton, after the lapse of several years, did not remember this fact, then Leibnitz too might forget it; for although indeed it was four years since Newton had written the letter to Collins, yet it was not four years since Leibnitz had been acquainted with the letter, and in the next place Leibnitz was the learner, who would pay attention to all that came new to him; whereas Newton was not, like a learner, attentive to the extent of what he communicated, and could quite forget having made this communication which after all he had sent only to Collins and not to Leibnitz.

That Newton's letter on Tangents had actually been known to Leibnitz, has been only rendered evident since 1849 by Gerhardt's discovery of the *abstract* which Collins had got ready for Leibnitz, containing the last part of this letter of Newton's upon tangents in the words: *quod scilicet Dn. Newtonus cum in literis suis* 10 *Decembris*, 1672, *communicaret nobis methodum ducendi tangentes ad curvas geometricas ex aequatione exprimente relationem ordinatarum ad Basin, subjicit hoc esse unum particulare, vel corollarium potius, methodi generalis, quae extendit se absque molesto calculo, non modo ad ducendas tangentes accommodatas omnibus curvis, sive Geometricas sive Mechanicas, vel quomodocunque spectantes lineas rectas, aliisve lineis curvis; sed etiam ad resolvenda alia abstrusiora problematum genera de curvarum flexu, areis, longitudinibus, centris gravitatis etc. Neque* (*sic pergit*) *ut Huddenii methodus de maximis et minimis, proindeque Slusii nova Methodus de tangentibus,* (*ut arbitror*) *restricta est ad aequationes, Surdarum quantitatum immunes. Hanc methodum se intertexuisse, ait Newtonus, alteri illi, quae aequationes expedit reducendo eas ad infinitas series; adjicitque, se recordari, aliquando data occasione, se significasse Doctori Barrovio, lectiones suas jam jam edituro, instructum se esse tali methodo ducendi tangentes, sed avocamentis quibusdam se praepeditum, quominus eam ipsi describeret.*

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In the next place Edleston attests that a copy of the whole letter was sent to Tschirnhaus in Collins's paper "about Descartes," and Gerhardt confesses that Leibnitz and Tschirnhaus, in 1675, worked together, and were so intimately connected that they used the same paper and the same ink and pen.*

Even before these most recent investigations, it was concluded from the silence of Leibnitz that the Newtonian letter on tangents was known to him; for this much had been distinctly affirmed in the *Commercium Epistolicum*, and Leibnitz did not deny it. Prof. De Morgan has discovered that in the first edition of the *Comm. Epist.*, which appeared in the lifetime of Leibnitz, this distinct affirmation and statement is only found in the judgment pronounced by the Committee, *nusquam mentionem reperimus Methodi ejus Differentialis*

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* Edleston's Correspondence, etc., page xlvii, and Gerhardt (I., page 91) and Tract II. of 1855, page 68. For superabundance we have Leibnitz's own words to prove his intimacy with Tschirnhaus, see Leibnitz's letter to Oldenburg of 28th December, 1675: Quod Tschirnhausium ad nos misisti, fecisti pro amico—inventa mihi ostendit non pauca Analytica et Geometrica. Leibnitz moreover, if Tschirnhaus had hesitated to show him what he had, could most easily see the whole letter when he was in London, in October, 1676.

Gerhardt, in his recent volume (Leibnitz Mathem. Schr. Band IV., 1859, page 420) says: "In May, 1675, Tschirnhaus was not in Paris, but either in London "or on his way to London." This remark, instead of assisting Leibnitz, as Gerhardt thinks, corroborates on the contrary Edleston's statements. Indeed what was sent to Tschirnhaus at that time, being altogether fourteen folio leaves, (Edleston, l.c.) it would be difficult to suppose that this was sent to Paris, but not difficult to conceive that it was sent to Tschirnhaus, while he was in London. Tschirnhaus received them in London and returned with them to Paris. Gerhardt states (ibidem, Vol. V., page 421) that Leibnitz's mathematical manuscripts of the year 1675 prove the intimacy of Leibnitz and Tschirnhaus: "on the same leaf (says Gerhardt) we find "the pen and handwriting of Tschirnhaus, together with the pen and handwriting "of Leibnitz."

Tschirnhaus's answer to Collins's paper is quoted in the Commercium Epistolicum as received June 8, 1675. Collins, in the "Extracts from Mr. Gregory's Letters," says, that Tschirnhaus was "here a quarter of a year in the summer of 1675." (I am indebted to the kindness of Mr. Edleston for this memorandum made by him when he examined the papers at the Royal Society.)

ante literas ejus (*Leibnitii*) 21 *Junii*, 1677, *hoc est Anno integro postquam D. Newtoni Epistola*, 10 *Decembris*, 1672, *scripta Parisios ipsi communicanda transmissa fuit*, while in another edition which appeared after Leibnitz's death the remark was also found in a second more conspicuous (?) place in the *Commercium Epist.* (on the occasion of the printing of this letter, *Missum fuit apographum hujus Epistolæ ad Leibnitium mense Junio*, 1676). This is an unheard-of thing! exclaims De Morgan.

This Prof. De Morgan could fairly do, as long as it was assumed, while he made this great discovery (Philos. Magazine, June, 1848) that Leibnitz had not seen the letter of 10th December, 1672; for then it was felt that Leibnitz's silence proved more against him, if the statement of the fact appeared twice in the *Commercium Epistolicum* than if it was only once there. But since 1849, now that we can no longer doubt that Leibnitz received the entire letter from Tschirnhaus, and the reference to the same from Oldenburg, this discovery of a variation in the statement about it, by which Prof. De Morgan has become celebrated, obviously loses all that immense importance which it may ever have had.

But instead of now giving up his attempt of proving a case against Newton, Prof. De Morgan asseverates yet more strongly (in the Companion to the British Almanac of 1852) that it is clear from Gerhardt, that it was not the letter of 10th December, 1672, but an abstract of the same, that was sent to Leibnitz direct, while the whole letter was sent to Tschirnhaus, and that hence the deceitful design of Newton and of the Committee was manifest.

Prof. De Morgan here perverts the case entirely. There is indeed an error in the *Comm. Epist.* There were in fact at the time of the *Comm. Epist.*, 1712, on the shelves of the Society, two rolls of Collins's bound together, one with the heading, *Extracts from Mr. Gregory's Letters, to be lent Mr. Leibnitz to peruse, who is desired to return the same to you*, (in which was contained the whole letter of 10th December, 1672), and another in which this letter was found

only in abstract, as Gerhardt gives it, this also having been inscribed by Collins, *To Leibnitz*, 14 *June*, 1676, *about Mr. Gregory's remains;* and the Committee, as well as Newton, assumed erroneously that the first roll had been sent to Leibnitz, instead of the second. But into this error Newton fell even in the lifetime of Leibnitz, because already in the first edition of the *Comm. Epist.* it was registered that the first named collection was to be lent to Leibnitz; the smaller collection, which gave this letter only in abstract, was not referred to at all. Therefore Leibnitz's death has nothing at all to do with the matter, and that which was said before Leibnitz's face may have been misstated through error, but cannot have been misstated with a deceitful intention. "*Something can be allowed for hurry,*" says Prof. De Morgan himself, "*for the Committee was appointed in parcels on March* "*6th*, 20*th*, 27*th*, *and April* 17*th*, *and their report was read as early as* "*April* 24*th.*"

So important is and has always appeared the question, whether Leibnitz read the letter of 10th December, 1672, that in order to excuse the silence of Leibnitz, Prof. De Morgan only a short time before Gerhardt's discovery, and so also Biot in the Journal des Savants following strictly De Morgan, made a great stir, because the affirmation that Leibnitz had seen the letter, came only once in that edition which appeared in Leibnitz's lifetime, and not twice, as in the second edition. Moreover we see even now Prof. De Morgan catching at a straw, and saying, that it was only an abstract, not the whole, of the letter that had been communicated to Leibnitz.

But now that since the year 1849, through the investigations of Gerhardt and Edleston, this whole matter is seen in a pretty clear light—Leibnitz having been so intimate with Tschirnhaus, that they worked on the same paper, and the whole letter having been sent to Tschirnhaus, and an abstract to Leibnitz, Leibnitz's advocates turn round, and say, after all the strife they themselves raised upon the subject before 1849, that it is of no consequence whether Leibnitz did read this letter of Newton's on tangents—"*a sheet of blank paper, after*

"*what Sluse had published, would have done just as well as the abridgment* "*or the whole,*" says Prof. De Morgan at the end of his essay of 1852.

The reader will smile at here seeing, that the same Prof. De Morgan would first have us consider the fact as so important, that an increase of reputation might be gained by it, and secondly so unimportant! "*Only one (document) is undated, and this is that on which the whole* "*turns,*" said Prof. De Morgan in 1848; and in 1852 he exclaims, "*a sheet of blank paper would have done as well.*" It is impossible to contradict one's self more glaringly.

But let us quit Prof. De Morgan, and cast a look backwards. It cannot have escaped observation, that we have hitherto not employed the word *fluxions*. It will be asked why. The answer is because Newton in his Compendium on the Calculus in 1669 has not once used this word. Also those supporters of Leibnitz are therefore mistaken, who sometimes introduce him as the discoverer of the Differential Calculus, and Newton as the discoverer of something else which they prefer to call "*Fluxional*" calculation; (as in Gerhardt's Tracts we find two separate chapters under the headings, Discovery of the "Fluxional" calculus by Newton, and discovery of the higher analysis by Leibnitz). The thing which the second comer did or did not discover, is not different from that which the first comer discovered. Newton gives (see above pages 21, 24) in his Compendium 1669 the rules of the calculation: *Regula* 1; *Si*

$$ax^{\frac{m}{n}} = y\,;\ erit\ \frac{an}{m+n}\,x^{\frac{m+n}{n}} = area\,;$$

he works this out in all examples, and says (under the heading: *Applicatio praedictorum ad reliqua hujusmodi problemata*)*: Jam qua ratione superficies ex momento suo perpetim dato per praecedentes regulas elicitur, eadem quaelibet alia quantitas ex momento suo sic dato elicietur. Exemplo res fiet clarior.* Then follow the examples under the heading, *Longitudines curvarum invenire*, among which we have the small (afterwards so called, Differential) triangle (see the figure above, page 22),

then we have in general the title, *Invenire prædictorum conversum.* The first proposition, *Si* $ax^{\frac{m}{n}} = y$; *erit* $\frac{an}{m+n} x^{\frac{m+n}{n}} = area = z$, is at last proved (at the end of the whole Treatise), where we read, *Si* $\frac{n}{m+n} ax^{\frac{m+n}{n}} = area = z$, *erit* (we have Newton's proof, the same as is now given, effected by giving the increment o to x and then putting $o =$ zero), *erit* $ax^{\frac{m}{n}} = y$. Then Newton says, *quare e contra si* $ax^{\frac{m}{n}} = y$, *erit* $\frac{n}{m+n} ax^{\frac{m+n}{n}} = z$. In this Newtonian discovery of the Differential Calculus, which is contained in the Compendium, the word *fluere* does not occur.

It is in 1676 that Newton, in his letter of the 24th October to Leibnitz, in the passage which was not legible for the latter, *data æquatione quotcunque fluentes quantitates involvente fluxiones invenire et vice versâ*, first uses the word *fluere*, in order to indicate succinctly the whole method of calculation employed in the Compendium.

CHAPTER IV.

BIOT'S JUDGMENT ON THE DISCOVERY OF THE DIFFERENTIAL CALCULUS.

M. Biot, as is well known, has expressed himself very specifically on these questions in the *Journal des Savants*, and 1856, in the new edition published by him and Lefort of the *Commercium epistolicum.* We are sorry to find that M. Biot everywhere quite echoes the words of Prof. De Morgan, which can only be explained by supposing, either that M. Biot purposely refuses to see how the thing stands, or that holding fast by the customary differential form and style of writing of the present day, he is unable to throw himself back into the past. It is an extraordinary thing, that Wallis is not mentioned in that part of the *Commercium Epistolicum* in which M. Biot and M. Lefort give the names of those, "whose labours paved the way for the discovery "of the infinitesimal Analysis ["*dont les travaux ont préparé l'invention* "*de l'analyse infinitésimale*"].

"*On s'étonnera peut être,* says M. Biot or his co-labourer M. Lefort, "page 254, *de ne rencontrer ni Wallis ni Huyghens. Cependant* "*Wallis et Huyghens ne me paraissent avoir aucun droit direct de paternité* "*sur les nouveaux calculs, qu'ils ont tous deux méconnus, le premier plus* "*encore peut être que le second.* [The reader will perhaps be astonished, "says M. Lefort, or M. Biot, page 254, at not meeting with either "Wallis or Huyghens. But Wallis and Huyghens do not appear to "me to have any direct paternal right in the new calculations, which

"they have, both of them, misappreciated, the first perhaps even more "so than the second."]

Thus we see that Wallis is made of so little consequence, that while persons remotely interested in the matter are named, such as Fermat, Cavalleri, Hudde, Sluse, and while these and even Descartes and Ricci are quoted from, in order to give specimens from those whose *travaux ont préparé l'invention au dix-septième siècle*, Wallis is not even admitted.

We on our side affirm that it is, above all, the works and the labours of Wallis, that the whole *Commercium Epistolicum* and Newton's letters to Leibnitz, as well as Leibnitz's letters, always presuppose and refer to. This Messrs. Biot and Lefort ought not to have overlooked, were it only for the *Recensio*. Therein Newton says: *Per infinitas aequationes intelliguntur illae, quae involvunt Seriem terminorum convergentium et ad veritatem propius propiusque accedentium in infinitum; ita ut postremo a veritate distent minus ulla data quantitate; et, si in infinitum continuentur, nullam omnino differentiam relinquant.*

Wallisius in Opere suo Arithmetico, publicato A.D. 1657, *Cap.* 33. *Prop.* 68. *reduxit fractionem* $\frac{A}{1-R}$ *per perpetuam Divisionem in seriem* $A + AR + AR^2 + AR^3 + AR^4 + etc.$

Vicecomes Brounker quadravit Hyperbolam per hanc seriem

$$\frac{1}{1\times 2}+\frac{1}{3\times 4}+\frac{1}{5\times 6}+\frac{1}{7\times 8}+etc.$$

hoc est per hanc $1-\frac{1}{2}+\frac{1}{3}-\frac{1}{4}+\frac{1}{5}-\frac{1}{6}+\frac{1}{7}-\frac{1}{8}+etc.$ *conjungendo singulos binos Terminos in unum. Et haec Quadratura publicata est in Actis Regiae Societatis, mense Aprili* 1668.

Paulo post Dominus Mercator evulgavit Demonstrationem hujus Quadraturae per Divisionem Domini Wallisii; et deinceps haud multo post Jacobus Gregorius Geometricam ejusdem Demonstrationem in lucem edidit. And further (*ibidem*): [*Newtoni compendium* 1669]—*continet praedictam generalem Methodum Analyseos, monstrantem quomodo resolvendae sunt finitae Aequationes in infinitas; utque per Methodum Momentorum appli-*

eandae sunt Aequationes tam finitae quam infinitae ad omnium problematum solutionem. Incipit vero, ubi finem fecit Wallisius, et methodum Quadraturarum super tres Regulas struit.

Wallisius Anno 1655, *Arithmeticam suam Infinitorum in lucem dedit; per cujus libri Propositionem LIX. si Abscissa cujusvis Curvilinearis figurae vocetur X, et n atque m sint Numeri, et Ordinatae ad rectos angulos erectae sint* $X^{\frac{m}{n}}$; *Area figurae erit* $\frac{n}{m+n} X^{\frac{m+n}{n}}$. *Atque hoc assumitur a D. Newtono, tanquam prima Regula, super quam fundat suam curvarum Quadraturam. Wallisius autem propositionem hanc demonstravit gradatim, per multas particulares propositiones; tandemque omnes in unam collegit per Tabulam Casuum. Newtonus vero omnes casus in unum reduxit, per Dignitatem cum indefinito Indice: et sub extremo Compendii, semel simulque demonstravit per Methodum suam Momentorum; primusque indefinitos dignitatum Indices in Operationes Analyseos introduxit.*

Ceterum per 108 *Propositionem Arithmeticae Infinitorum Wallisii, perque plures alias propositiones quae sequuntur; Si ordinata composita fuerit ex duabus vel pluribus ordinatis cum signis suis + et − acceptis; Area composita erit ex duabus vel pluribus areis cum signis suis + et − acceptis respective. Atque hoc a D. Newtono assumitur, tamquam Regula secunda, super quam instituit suam Quadraturarum methodum.*

Hence we see that it is not by mere accident, like almost all those whom Biot and Lefort mention as being important, but much more directly, that Wallis comes into the question, if we look back at the position of this branch of knowledge immediately before the discovery, as Messrs. Biot and Lefort wish to do, at least as they tell us.

In the next place it becomes very clear from the commencement of Leibnitz's letter to Newton of the 27th August, 1676, that the Wallisian simple beginnings of quadratures, (Wallis's method, we might say, of integration) are the foundation of the correspondence between

Leibnitz and Newton. For after the exposition of Leibnitz's method of transmutation, which then appeared to him a correct one, Leibnitz says, *Unde ad quadraturas absolutas, vel hypotheticas Geometricas, vel serie infinitâ expressas Arithmeticas jamjam multis modis potest perveniri.* What other are these methods of quadrature, which are nowhere described, and which are therefore assumed as known, but those of Wallis? Indeed Leibnitz immediately after says downright *positâ* β *infinite parvâ*, an expression and an idea which Wallis has, and if any one will not admit that Leibnitz took this out of Wallis, it would be for that very reason more necessary for him to mention at least that Wallis has also this expression.

In the second letter to Leibnitz, Newton again says expressly that it was through the interpolation problem, which Wallis had instituted, that he himself had been led to his new invented method. Here Newton continues speaking to Leibnitz of these *opera* of Wallis's for many pages together, and takes it for granted that they are known to Leibnitz, and speaks of them as the foundation of the process for effecting the quadrature. Finally Leibnitz, to name him too once more, begins his letter to Newton of the 21st June, 1677, with the words, *Egregie placet, quod descripsit qua via in nonnulla sua elegantia Theoremata inciderit* (*Newtonus*), *et quæ de Wallisianis interpolationibus habet, vel ideò placent, quia hac ratione obtinetur harum interpolationum demonstratio, cum res ea antea* (*quod sciam*) *sola inductione niteretur.* It is obvious that the inductions and the quadratures of Wallis are everywhere presupposed by Newton as well as by Leibnitz as a foundation.

Messrs. Biot and Lefort, who name and quote from Fermat, Cavalleri, Hudde, Sluse, and even from Ricci and Descartes, as *auteurs qui ont preparé l'invention au dix-septième siècle* make thus no mention of Wallis, and the grounds on which they justify themselves are, that "*Wallis et Huyghens, ont le premier plus encore que le second, méconnu* "*ces nouveaux calculs* [*Wallis and Huyghens have misappreciated,* "*the first even more than the second, the new calculations*]." This is doubly incorrect. Even if it is substantiated, that Huyghens, after the

discovery had been made, did not immediately understand it, or that he did not acknowledge it, yet he ought not on account of this *ex post facto* behaviour of his after the discovery of the Differential Calculus to be excluded, but for another reason, namely, because he had not before taught anything that in the main or somewhat nearly resembled the Differential Calculus. Descartes and Fermat also misappreciated the Differential Calculus, that is, they did not understand it at all, because they were not at all acquainted with it, and yet these writers are quoted from. The question, does not turn upon what an author said after the discovery, but upon what he said beforehand that was like it. But further, though it be true of Huyghens, that he was not favorably disposed to the Differential Calculus, (though this is not the ground on which he should be excluded,) yet the fact does not stand thus with Wallis, because it was he himself in 1693, who in his treatise *de Algebra* highly extolled the calculus invented by Newton, and published it before Newton himself.

The justice of Wallis's title is, we hope, distinctly clear to the reader, because we have given in Chap. II. Wallis's quadratures by integrations and summations in the simple cases which he could master (of the others he said honestly hic haeret aqua, thereby exciting and forcing Newton).

We can admit that Biot, amidst his present multifarious occupations, is not answerable for the book which appeared in 1856 under his name and the name of the real author M. Lefort, this Biot tells us in the Preface: "*l'exécution appartient tout entière à Mr. Lefort;* "*et il s'est acquitté de cette tâche—avec une puissance de travail, que* "*je suis heureux de reconnaître, mais qu'il m'aurait été impossible d'y* "*apporter.* [The execution belongs entirely to M. Lefort, and he has "acquitted himself of this task with a diligence, which I am thankful "to acknowledge, but which it would have been impossible for me to "have exercised."]

But M. Biot himself in the Journal des Savants for 1832, forgets the claims of Wallis; his words are (page 267, line 2): "*la première*

"*lettre de Newton à Leibnitz contient les résultats de Newton sur les* "*séries, notamment la formule du binôme, le tout sans aucune démon-* "*stration ni indication de méthode quelconque, disant seulement, qu'il* "*en possède une à l'aide de laquelle ces séries étant données, il peut* "*obtenir les quadratures des courbes dont elles dérivent.* [The first "letter of Newton to Leibnitz contains the results arrived at by "Newton in reference to series, particularly to the binomial theorem, "the whole without any demonstration or indication of a method, "saying only that he is in possession of one by which these series "being given, he can obtain the quadratures of the curves from which "they are derived."] This is not correct. The parts of Newton's method there in question are not only, I. the reduction of complicated equations to series of the powers of x which he openly communicated, and II. a method which he concealed of effecting the Quadrature of these series, for the Quadrature of equations reduced to simple powers was a thing understood of itself, and that could not be kept back, because Wallis had long ago given this in his works, which were well known to everybody.

So it happens that Biot, because the method of tangents of Barrow and Newton does also not seem important to him, inasmuch as Descartes and Fermat, had had another, comes to this perverse conclusion in reference to the whole matter, that we must assume "*trois* "*phases de l'invention bien marquées: 1° emploi des évanouissans comme* "*méthode aux fonctions rationelles, ceci appartient spécialement à Fermat;* "*2° extension aux fonctions irrationnelles par le développement en série,* "*surtout au moyen du théorème du binôme, voilà la part spéciale de* "*Newton; 3° réduction de cet artifice particulier en méthode générale* "*de calcul, voilà Leibnitz,* [three well-marked phases of the inven- "tion, 1. the employment of vanishing quantities as a method for "rational functions, this belonging especially to Fermat; 2. the ex- "tension of this to irrational functions by development in the form "of a series, especially by means of the binomial theorem; this being "the special part of Newton; 3. the reduction of this particular

"artifice to a general method of calculation, in which lies Leibnitz's "invention."]

Thus Newton is made to have discovered only *une part spécielle* and *un artifice particulier*, [a special part and a particular artifice], but Leibnitz *une méthode générale*, a general method; in other words, Biot will allow Newton only the *développement en série*, and not the general calculation *datâ æquatione, quotcunque fluentes quantitates involvente, fluxiones invenire et vice versa.* We see that Newton is taken into high company and must content himself, according to the judgment of M. Biot, with only that modest place which Fermat and Leibnitz just leave between them.

This judgment of Biot's however is quite unsound, because Fermat hardly deserves to be named at all, and secondly, because Newton's Compendium, the Analysis of 1669, contains not only the *dévéloppement en série* but also the *méthode générale*, which consequently is not in any way left for Leibnitz, if we leave out of consideration that he was perhaps not acquainted with Newton's discovery.

Indeed, if Biot had said only that Newton's discovery of a general method was not to be taken into consideration, because Leibnitz had not seen it, then the question would turn upon this, whether the communication of the method of tangents, and whatever else Newton communicated to Leibnitz, was or was not sufficient to deprive Leibnitz of the honour of the discovery. But M. Biot not only says that nothing whatever but the method of series was communicated to Leibnitz, but he says that Newton had also discovered nothing else, which is tantamount to saying, that Newton in his letter to Leibnitz concealed nothing, and did not write his anagram, data æquatione, etc.

The ground of the exclusion of Wallis and of the higher eulogium of Leibnitz, lies evidently in the design of making Fermat a co-discoverer of the Differential Calculus. It is necessary to throw dust in people's eyes, in order to make this long ridiculed crotchet of the French wear again for a moment a serious appearance. It has been ridiculed, I repeat it, not only in England, but also always

in Germany! But evidently this was Biot's design; with this view he pronounces an unfair judgment against Newton, and with this view he and Lefort publish a comic edition of the *Commercium Epist.* with a French sauce; M. Biot sees that the strife between England and Germany is being renewed; "therefore we in France must "put in our word", said M. Lefort to M. Biot, or said M. Biot to M. Lefort, and so they got up an edition with clever addenda, in order to thrust in Fermat in an unsuspicious manner. Biot gave the capital of his reputation, and Lefort the capital of his labour for this joint-stock concern, in which the little various readings of the first and second editions of the *Commerc. Epist.* are taken advantage of, in order to have an excuse to set aside Wallis* and to push in Descartes and Fermat.

* The excuse *on s'etonnera peut être de ne voir ni Wallis ni Huyghens, qui ont tous deux méconnu les nouveaux calculs* is characteristic of the whole book. The idea, which this phrase is designed to convey, is that Huyghens (who was certainly not a Frenchman, but who—having lived in Paris and having been received into the *Academie des Sciences* at the time of its foundation—in order that it might not be composed of only minor celebrities—has been ever since looked upon by the French as a fellow-countryman,) might appear in the same degree as Wallis, an Englishman, to have prepared the way for the discovery which was then impending, and that the omission of the one in Biot and Lefort's book balances that of the other. Even connoisseurs, if they do not keep a strict watch over their memories, are liable to be lulled asleep by these smooth phrases, especially because Huyghens was so very celebrated on other accounts. If the reader does not allow the phrase to domineer over his consciousness, then he thinks that the less important Huyghens was left out in order to justify the omission of Wallis. At the third stage of reflection one gets hold of the idea, that Huyghens never came under consideration, not even in Biot and Lefort's idea, and that he is only emphatically named here; so then one perceives that to be the best interpretation which credits the author of these lines with the greatest share of French ingenuity; for although Fermat and Descartes are thrust in, yet because the date of their discoveries is so very remote, it might annoy a Frenchman to find all more modern countrymen of theirs, (Members (!) of the Parisian "Academie des Sciences", which had been in existence since 1666,) not even mentioned by name in Messrs. Biot and Lefort's Book;—on this account they here at least introduce Huyghens, who was celebrated on other accounts, and who lived in Paris. Thus we kill two birds with one stone; we get rid of Wallis and have mentioned Huyghens.

Biot's conclusion is therefore false, and the case remains thus, that Newton's invention and Leibnitz's are the same, and that Newton made the invention earlier than Leibnitz. The celebrated Frenchman Montucla, who weighs more in the scale of historical mathematics than any Frenchman, says, after having discussed all the facts in detail, (page 109, vol. 1), "*il est temps de nous résumer, et d'abord on ne peut* "*douter, que Newton ne soit le premier inventeur des calculs dont il* "*s'agit, les preuves en sont plus claires que le jour.*" ["It is time we "should sum up, and in the first place it cannot be doubted that Newton "was the first discoverer of the Calculus in question; the proofs of "this are clearer than daylight".] Why does not M. Biot cite this French writer?

We believe, however, that we have proved, not only that Newton was the first discoverer, but also that Leibnitz, inasmuch as Newton's method of tangents was known to him, had no right in his letter of 1677, to write *arbitror quæ celare voluit Newtonus de Tangentibus ducendis a meis non abludere*, but that he ought to have written, *quæ celare voluit Newtonus mihi nota sunt, nam literas ejus* 10*mi Decembris* 1672 *inspexi.* This is the verdict which those who in 1712 edited the *Commercium Epistolicum* annexed to their edition, a verdict which at the time when it was given was doubtful, because it was new, and because Leibnitz was in possession of the honour of the invention, which grew still more doubtful, as the notation of Leibnitz (*dx* and *∫dx*) became general, whereby the difficulty of understanding the question was increased, but which now since the fact of Leibnitz's having read with Newton's open communications, also Newton's letter upon Tangents, has been recently since 1849 established, has become in the eye of impartial readers a safe verdict.

CHAPTER V.

A NEW FEATURE IN THE QUESTION.

We have shewn that Leibnitz's merits, owing to his having seen Newton's letter of 1672, which he professed to be ignorant of, were smaller than he claimed, inasmuch as Newton's method of tangents together with his and Barrow's demonstration of the same, make up that which is called the "independent" discovery of Leibnitz. But we regret to say, that the matter perhaps does not end here. For we are alarmed at hearing Gerhardt naïvely tell us, that he has upon his table a manuscript, which he got out of the Hanoverian library, in Leibnitz's handwriting, without date, and headed, *Excerpta ex tractatu Newtoni Manuscripto de analysi per æquationes numero terminorum infinitas.* What are we to think of this? And still worse, Gerhardt adds hereto that in his opinion Leibnitz cannot have seen these extracts from Newton's compendium between 1672 and 1674. So he forgets altogether that Leibnitz ought never to have seen this paper at all, if a tittle of his reputation is to remain with him.

If we do not deceive ourselves this is Gerhardt's meaning,* that because in Leibnitz's extract we find the sign [*∫y*], as the now so well-known mark of integration, while however the differential sign [*dy*] is not found, (for of the latter Gerhardt makes no mention,)

* We give Gerhardt's words as the first Addendum.

therefore Leibnitz may or must have made these extracts certainly not before 1674, but before 1677 (in 1675–76).

The French also, with their usual acuteness, have so understood the matter. For Biot and Lefort say at the end of their edition of the *Commercium Epistolicum*, (page 290) "*Il est possible que ces extraits* "*(de Leibnitz) aient été pris sur le manuscript de Collins, pendant le* "*séjour (de Leibnitz) d'une semaine à Londres en Octobre* 1676 *; mais* "*leur contexture ne permet pas de douter qu'au moment de la trans-* "*cription Leibnitz ne fût en possession des éléments du calcul intégral.* "[Possibly these extracts of Leibnitz were taken from the manuscript "of Collins, during the week's stay that he (Leibnitz) made in London "in October, 1676; but their context does not leave us room to "doubt, that at the moment he transcribed them, Lebnitz was in "possession of the elements of the integral calculus."]

Mark that the word *integral* calculus, and not the word differential calculus, is used here.

In this very delicate question a word is of consequence, and M. Lefort knew right well that the most natural word was differential calculus; as Gerhardt knew, that if by the side of the sign [$\int y$], he could have found the sign dy, this ought also to have been mentioned with the other. We may therefore venture to say that Gerhardt and the French editors intimate, that Leibnitz indeed made these extracts, after his discovery of the *Integral Calculus*,* but yet before his discovery of the *Differential Calculus*.

* They imagine that Leibnitz discovered the integral calculus, when they find him put down the sign $\int y$. In truth expressions, such as in Gerhardt (page 58, tract of 1855,) *omne l—omn. omn. l.*, and the substituted *S pro omni*, are nothing more than those we meet with in Wallis and De St. Vincent, *ductus plani in planum (quod appellat ille Vincentius) plani in planum ductum in meo (Wallis) tractatu dicitur ductus rectarum omnium unius plani in alterius rectas.* (Preface to the Arith. infinit.) The idea that a surface = *omn. y* is indeed the entire import of the Wallisian summations, in order thereby to measure the surface. Hence Wallis or even Cavalleri is the discoverer of the integral calculus, as far as this can be recognized without the accompaniment of a Differential Calculus. In his Mathesis, or *Arithm.*

How is this possible?

We must here say something of the relations of Leibnitz to Collins, or more correctly speaking, to Oldenburg. The Royal Society in London had committed the oversight of employing as their secretary, not an Englishman, but a German named Heinrich Oldenburg. This imprudence could not but soon have its consequence, and this consequence in particular, that when once the right man came, the interest of England was more or less sacrificed to a German friendship. We say here nothing against Oldenburg, for he knew not what he did, and Leibnitz did but take advantage of this situation. There soon arises a friendship between them. We lack the first letters directed to Oldenburg. Leibnitz, supported by his patron, the Baron of Boineburg, whom Oldenburg had long been acquainted with, must have known how to hit the proper tone; for so early as 5th August, 1671, Oldenburg writes to him,* while committing letters for Germany to a noble friend of his: *dimittere harum gerulum nobilissimum non potui, quin Te salutarem, simul et fidem facerem, me reliqua quae de me exspectas, quam primum fieri id poterit, confecturum. Caeterum cum eximius Helmontius, affectu mihi conjunctissimus, propediem ad nos sit reversurus, poteris si placet, ipsi tuto committere, quaecunque forsan mihi scribenda vel communicanda occurrerint.* What are these *reliqua quæ de me exspectas?* and what could Leibnitz have to say to Oldenburg, for which it ap-

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universalis of 1657, Wallis puts down for such additions of progressions *terminorum summa vel aggregatum* = *S*. See *Opera*, edition of 1699, *Tom.* I. page 140. Leibnitz may now and then have believed that he had made a discovery by speaking of omn. omnl. and S pro omni, but he must soon have perceived the contrary, because he does not again speak of his sign S pro omni, that is, of his and other people's idea of summing series either in one of his letters to Newton of these dates or anywhere else. To add to Wallis's ideas of summation the differential calculus, that was the discovery. The sign [$\int y$] therefore means nothing if the sign [dy] and the idea which can be laid into this latter sign, and in others of its kind, is not found.

* We of course quote everywhere from Gerhardt's Edition of the Letters.

peared expedient to name Helmontius as his friend, to whom anything could be communicated by word of mouth or by letter so securely. Leibnitz, like many young men of his age, was over-desirous of acquiring a great reputation, and the Secretary being a German was to assist him. This was the bargain that was soon and perhaps almost naïvely struck between them. Thus at once in the letter of the 12th June, 1671, we see the Secretary so interested in the "circulation" of a Leibnitzian paper, that he has it printed. *Cæterum, vir amplissime, morem gessi desiderio tuo, et pro commodiore distributione scriptum tuum hic recudendum tradidi.* (Gerh. page 22.)

That Oldenburg in 1671 did not yet believe that Leibnitz was already a great man is proved by his words in the letter of December, 1670. (Gerh. page 16): *Finem hic facerem nisi ad Epistolae tuae calcem de Motus perpetui procurandi ratione perquam facili, a Te inventa, nonnulla innueres.... Ais Te rei demonstrationem expedivisse—Facile, puto, credes, me in Anglia peregrinum, sine palpo et assentatione de Anglis pronuntiaturum. Sunt inter eos viri complures, subacto in rebus Mathematicis et Mechanicis judicio praepollentes, quorum de invento isto tuo sententiam ut exquiras, prius quam id evulges, ejusve Actorem te scribas, omnino et amice suaserim. Si consilium allubescat, meque hac in re parario opus fuerit, provinciam non detrecto, omnemque quae virum bonum decet candorem spondeo.* How delicately Oldenburg here labours to avert the danger of his friend falling into ridicule, and offers himself as a *pararius*, or agent, whom, as not being an Englishman, Leibnitz can trust. But Oldenburg was appointed by the English, in order that he might protect the honour of the English, and not that he might be the agent of a foreigner, and give him clandestine support.

Leibnitz followed the advice of Oldenburg, and escaped the ridicule of introducing himself as the discoverer of the impossible *perpetuum mobile;* but when he came to London in 1672 he had the lighter misfortune of twice giving out as his own invention what was already to be seen in print. We refer to the well known anecdote, that Leibnitz

in a party at the house of Boyle, ventured to say that he had discovered a certain method for employing the subtractions of square roots, whereupon Dr. Pell cited to him Mouton, in whose works this was to be read. Oldenburg contrived a defence for Leibnitz, in which the latter at last added, (Gerhardt, page 31) that he had something else, namely, a method (*methodum habeo*) of summing fractional series (*summam inveniendi seriei fractionum in infinitum decrescentium, quarum numerator unitas, nominatores vero numeri triangulares aut pyramidales aut triangulo-triangulares*). We cannot help saying that the next passage in the correspondence, excites a suspicion that Leibnitz here commits a plagiarism, in the close of that very representation, by which he defends himself from the suspicion of another plagiarism. For it can scarcely be supposed that Leibnitz was not acquainted with the book of Mengolus, published in Germany, (and at Bonn,) in which these summations are given; the fame of which work had penetrated as far as England; and Leibnitz's words, when he was taunted with this plagiarism: *cum nondum mihi inquirendi in Mengolum otium fuerit*, (page 46) *et cum Mengoli liber non sit ad manus*, page 48, do not even contain a downright affirmation, that he was not acquainted with it. Oldenburg here again contrives his defence, and as Leibnitz had now quite become his pet and favourite, he exerted himself for his fame more than for his own, (very naturally, for Oldenburg himself could certainly not pretend to be a great mathematician, it was difficult enough to get his countryman and friend into reputation) and so we see with astonishment the endeavours of the two friends quickly crowned in the access of the young man to the honour of becoming a member of the Royal Society. The unusual request, which Leibnitz had addressed to the Royal Society, was couched in the terms that had been agreed upon between him and Oldenburg, (*voti coram Te expositi*, page 33); and so this weighty transaction was concluded just as they had wished, and Oldenburg was further to promise, by word of mouth, that Leibnitz would make every exertion in order that the Society might never repent of having complied with his request.

So it was by Oldenburg's exertions that Leibnitz had been received into the Society, without, at the time of his reception, having been preeminently qualified by his merit. This Leibnitz himself allows and admits when he says (see Gerhardt's Tract of 1848, pages 29 and 30, line 2; *cum Parisios appulissem anno Christi* 1672 *eram......... in superbâ pene dixerim Matheseos ignorantia*, and in Desmaiseaux, (Recueil II., pages 5, 114) "*au premier voyage en Angleterre, je n'avois* "*pas encore la moindre connaissance de la Géométrie avancée;* [on my "first journey to England I did not yet know anything of advanced "Geometry."]

We see that it was not in the few months that he stayed in Paris, before his first journey to London, 1672, but afterwards, that Leibnitz learned what was necessary. In order, nevertheless, that he might continue to play the part of a great man which he had begun to play in 1672, we see him in London and after his return from London come always upon the stage as a discoverer. Even Guhrauer, the professed biographer of Leibnitz, is not fascinated with this trait in his hero's character, and says in one case (Vol. 1, page 329), "one cannot help "placing the universal Characteristic, or the Philosophical Calculus, "which Leibnitz was in search of and attempted to discover, on a "level with the finding of the philosopher's stone, and the manu- "facture of gold. And with regard, so continues Guhrauer, to another "but purely mathematical project of Leibnitz's, the ANALYSIS SITUS, "Kant, whom no reputation (Guhrauer throws this in) could dazzle, "leaves it an open question, whether the cause of its non-completion, "was that Leibnitz thought his attempts as yet too imperfect, or that "the case was with him as it has been, according to Boerhaave, with "several great chemists, who gave themselves out to be possessed of "secrets, when they had really nothing but a persuasion and a con- "viction of their capacity for acquiring such; and thought that they "could not possibly fail in the execution, if they would once choose "and attempt it; at all events it appears as if the mathematical "discipline, to which Leibnitz gave by anticipation the title of *Ana-*

"*lysis situs*, and of which Buffon has bewailed the loss, had never "been anything but a chimera." Man kann nicht umhin, "die allgemeine Characteristik, oder den philosophischen Calcul, (den Leibnitz "suchte und ertinden wollte), mit dem Steine der Weisen und der "Goldbereitung auf eine Linie zu stellen; bei einem andren, aber rein "mathematischen Entwurfe Leibnitzen's der *analysis situs*, lässt Kant, "welchen kein Name blendete, [sagt Guhrauer], 'es dahingestellt, ob "'die Ursache der Nichterfüllung dahin zu setzen, dass dem Leibnitz "'seine Versuche noch zu unvollendet schienen, oder ob es ihm ge- "'gangen sei, wie Boerhave von grossen Chemisten vermuthet: dass "'sie öfters Kunststücke vorgaben, in deren Besitze sie wären, da sie "'eigentlich nur in der Ueberredung und dem Zutrauen zu ihrer "'Geschicklichkeit standen: dass ihnen die Ausführung derselben nicht "'misslingen könnte, wenn sie einmal dieselbe übernehmen wollten; "'wenigstens habe es den Anschein, dass jene mathematische Disciplin, "'welche Leibnitz im voraus *Analysin situs* betitelt, und deren Verlust "'unter Andern Buffon bedauert hat, wohl niemals etwas mehr als "'ein Gedankending gewesen sei.'"

This is Kant's verdict. With a character of this kind—"a burn- "ing desire for fame [einer brennenden Begierde nach Ruhm"], as Gerhardt terms it (Vol. 1, page 3), it may be easy for a man to think himself the discoverer of something which has already been discovered, and one knows not where this rage for discovery will be arrested. In England Leibnitz, as we know, had already twice been unlucky with his inventions, (hence perhaps a certain spite against the country); we refer to the discovery with which he came out in the Soirée at Boyle's, and to the other with Mengolus. But Oldenburg had defended him, and he had returned to Paris.

The first communication from Paris of any importance is contained in Leibnitz's letter of the 26th October, 1674, in which he writes to Oldenburg, just after mentioning some general investigations which appeared to him of not much value: *majoris ad usum vitae momenti est Profectus Geometriae; et inprimis Dimensio Curvilinearum: unde saepe*

praeclara Problemata Mechanica pendent. In ea Geometriae parte rem memorabilem mihi evenisse nuncio. Scis D. Vicecomitem Brounkerum, et Cl. virum Nic. Mercatorem exhibuisse Infinitam Seriem numerorum rationalium, spatio Hyperbolae aequalem. Sed hoc in Circulo efficere hactenus potuit nemo. Etsi enim Ill. Brounkerus et Wallisius dederint numeros rationales magis magisque appropinquantes; nemo tamen dedit progressionem numerorum rationalium, cujus in infinitum continuatae summa sit exacte aequalis Circulo. Id vero mihi tandem feliciter successit: inveni enim seriem Numerorum valde simplicem, cujus summa exacte aequatur Circumferentiae Circuli; posito Diametrum esse Unitatem. Et habet ea series id quoque peculiare, quod miras quasdam Circuli et Hyperbolae exhibet harmonias. Itaque Tetragonismi Circularis Problema, jam a Geometria traductum est ad Arithmeticam Infinitorum, quod hactenus frustra quaerebatur. Restat ergo tantum, ut Doctrina de Serierum seu Progressionum numericarum summis perficiatur. Quicunque hactenus Quadraturam Circuli exactam quaesivere, ne viam quidem aperuere per quam eo pervenire posse spes sit, quod nunc primum a me factum dicere ausim. Ratio Diametri ad Circumferentiam, exacte a me exhiberi potest per Rationem, non quidem Numeri ad Numerum (id enim foret absolute invenisse); sed per rationem Numeri ad totam quandam Seriem.

Oldenburg answers Leibnitz (who, as we perceive, stated himself to have discovered a special quadrature of the circle by approximation) saying that the English had not only this, but also a general method, by which to discover the same, and much else that was therewith connected. Oldenburg says: *ignorare te nolim, Curvarum dimetiendarum rationem et methodum a Gregorio nec non ab Isaaco Newtono ad curvas quaslibet, tum Mechanicas, tum Geometricas, quin et circulum, se extendere; ita scilicet ut si in aliqua curva ordinatam dederis, istius methodi beneficio possis lineas curvae longitudinem figurae aream et alia invenire.*

Now the full extent of this general method is no other than that which was afterwards termed the Differential Calculus; and no one

denies that if Leibnitz knew the Compendium on that subject of 1669, which Collins and Oldenburg possessed, this would be sufficient to convict him of plagiarism. And so indeed Leibnitz's curiosity was in the highest degree excited by the notion of the existence of this general method in the reach of his friend Oldenburg.

That Leibnitz himself had no sort of general method, of which his single quadrature was a special application, is evident from his letter. Besides which Huyghens writes just at this time to Leibnitz, (Gerhardt II. S. 16): *je vous renvoie, Monsieur, Votre escrit touchant la Quadrature Arithmétique que je trouve fort belle—Pour ce qui est de la ligne courbe Anonyme qui sert a Vostre demonstration.* Thus Leibnitz had no method of discovering nor of demonstrating quadratures, but Oldenburg excited his curiosity and described to him almost the very paper which Newton wrote, and of which not only Collins but also Oldenburg had copies in their desk, we mean the *analysis per aequationes numero terminorum infinitas*, in which the whole method of series and the Differential Calculus is found. Also in other letters of Oldenburg's since the year 1669, Oldenburg cites not only abstractly but with specimens this analysis, (compare *Com. Epist.* No. XIII.) We see that Newton made an exception to the fashion of his contemporaries, of publishing only bare results, he gave away his whole method in this compendium.

What now could Leibnitz do, when he became aware of this immense scientific wealth of the English? He might either beg that this method be communicated to him, or he might answer, "I will not "have your methods." But a man like Leibnitz, who is "burning" with the desire to gain fame, does not act in such a simple manner: he chose not only to learn these methods at whatever price, but also to appear as if he did not stand in need of them; he accordingly wrote to Oldenburg in the letter which Oldenburg could show to Newton (Gerhardt, No. XXIII.) *scribis clarissimum Newtonum habere Methodum exhibendi quadraturas omnes, omniumque curvarum Superficierum et Solidorum ex revolutione genitorum Dimensiones, et Centrorum*

Gravitatis inventiones, per appropinquationes scilicet, ita enim interpretor. Quae Methodus si est universalis et commoda, meretur aestimari; nec dubito fore ingeniosissimo auctore dignam. Addis tale quid Gregorio innotuisse; but on the same date he wrote the subsequent No. 24, *Mittam TIBI inventum meum, satis certe memorabile, quod magnitudinem Circuli per seriem numerorum rationalium infinitam mire simplicem exprimit: si mihi vicissim duo vestratium inventa Geometrica pollicearis, unum Collinii, de quo aliquando mentionem fecisti, de summis serierum numericarum finitarum, quarum termini sint primanorum, secundanorum, tertianorum etc. reciproci; alterum Gregorii circa methodum appropinquandi ad veram Circuli et Hyperbolae magnitudinem per series convergentes, cujus in Exercitationibus Geometricis exempla dedit. Et vero si Collinianum mihi consensu Clarissimi autoris, cui plurimam salutem a me dicas rogo, miseris quamprimum (nam etiam editum prostat, nisi fallor in libro quodam Anglico) statim transmittam meum et Gregorianum praestolabor, dum TIBI commoditas oblata fuerit obtinendi ab autore; neque enim credo Londini agit.*

Intelligo autem non inventa tantum, sed et demonstrationes mitti debere. Meum exactissime demonstratum, sed et numeris comprobatum habeo, et visum est ita memorabile insignibus quibusdam Geometris, ut inventorum Cyclometricorum hactenus cognitorum apicem appellare non dubitaverint.

This matches very well for Leibnitz who had now already learnt that series like his own were already printed in Gregory, (only without the method,) and could not therefore hope to shine by the side of Newton with his one poor quadrature, however he might dress it up; before the secretary however it was still feasible to glorify this invention; and it was this accordingly that he wanted to exchange with Oldenburg for the general method, about which Oldenburg was to enquire, not from Newton, but from Gregory, (*cui innotuit hæc methodus,*) that is, he was to get oral information, for which reason the business was to be adjourned awhile, viz. till Gregory came to London. Thus it was most cleverly pre-arranged that Oldenburg was

not to write upon the subject, but to serve Leibnitz by oral enquiries. It agrees perfectly with the diplomatic address of the Advocatus and Staatsrath Leibnitz, in writing to his friend Oldenburg, who himself had been a consular agent, that he does not bluntly say, "you know "on what terms we are with one another; my invention is a small "thing; get me secretly the greater one that your people have;" Leibnitz was not writing to a spy, whom he had bribed with money, but to a friend, of whom he only required, that he should do a small service to a fellow-countryman, without knowing how great was that service. But the import is not the less unfair; and we must ask whether after that No. 23, which Oldenburg deposited in the archives of the Society, Gerhardt's No. 24 could be anything but a private letter of the same date; for in No. 23 and in No. 24, the same subject is brought forward, though indeed in different ways. Compare the expressions in the two letters. At no other time was the position of things in regard to this single quadrature on the one side, and the Method of Quadratures on the other, such as is pre-supposed in this letter, which Gerhardt has therefore inserted in its present place, and could not have inserted elsewhere.

We will now at once remark, that any one, who would serve Leibnitz better than Gerhardt serves him, may here say, Gerhardt does not understand these things; he gives us documents, which are not that for which he passes them. No. 24 cannot be, as Gerhardt makes out, a letter supposed to be despatched, but only a draft, which never went to England. But with all the partiality that this view implies, it makes the case for Leibnitz not better than before; for it shows that Leibnitz had the design to get himself systematically informed about this method, *i.e.* the Differential Calculus, by means of oral enquiries, (according to the draft letter No. 24); while instead of it he sent off No. 23, by which he concealed that desire; perhaps because he did not feel quite sure that Oldenburg, to whom he could not yet speak by word of mouth, would serve him. The curiosity and the unallowable disavowal of this curiosity, and

the wish of obtaining through Oldenburg a secret information, remain proved, though our interpretation has tried to assist Leibnitz better than Gerhardt.

On the 20th of May, 1675, Leibnitz writes: *Cum nunc praeter ordinarias curas Mechanicis inprimis negotiis distrahar, non potui examinare series quas misisti ac cum meis comparare*, which would again be a falsehood, meant to conceal Leibnitz's desire to get informed about the English method, if it be (as we think it is) true, what is proved by Gerhardt's documents, (Tract of 1848, p. 23, note **,) that Leibnitz, at this very time, was not at all occupied with mechanical labours*.

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* In order not to break the thread of our investigations we will here just cursorily introduce an example of Leibnitz's anticipated discoveries out of the Correspondence, which confirms Kant's severe verdict respecting him, and shows at the same time that Leibnitz wanted to exchange that which he had *not* got hold of for something more substantial. He writes 12th June, 1675. *Ego rem molior, et satis credo in numerato habeo, qua nescio an ad usum major possit sperari in Algebra, methodum scilicet, per quam omnium Aequationum radices instrumento quodam, sine ullo calculo (post Aequationum praeparationem non difficilem) in numeris pro instrumenti magnitudine quantumlibet veritati propinquis, haberi possint. Si Collinius aut Parius inventum supradictum communicare voluerint, ego meum inventum, nemini hactenus a me monstratum, vicissim ipsis patefaciam.* Oldenburg answers and gives in six long numbers, all of what Collins had supplied him with, and this: Dn. Newtonus (ut hoc ex occasione literarum suarum, meaning Collins, addam) *beneficio Logarithmorum graduatorum in scalis παραλλήλως locandis ad distantias aequales, vel Circulorum Concentricorum eo modo graduatorum adminiculo, invenit aequationum radices. Tres Regulae rem conficiunt pro Cubicis; quatuor, pro Biquadraticis: In harum dispositione, respectivae coefficientes omnes jacent in eadem linea recta, a cujus puncto, tam remoto a regula prima, ac graduatae scalae sunt ab invicem, linea recta iis super extenditur, una cum praescriptis consentaneis genio aequationis, qua in regularum una potestas pura datur radicis quaesitae. Lubenter equidem cognosceremus, num Tu, Vir Doctissime, et Newtonus noster in artificium idem incideritis.* But now that Leibnitz has got all he wants out of the English, and can profit nothing further by them, he breaks off the subject with the words, *Methodum Celeberrimi Newtoni, radices Aequationum inveniendi per Instrumentum, credo differre a mea. Neque enim video in mea quid aut Logarithmi aut Circuli Concentrici conferant. Quoniam tamen rem vobis non ingratam video;*

In the letter of 12th May, 1676, Leibnitz had once more an opportunity of enquiring, quite without offence, just cursorily, what the English method might be; and when Newton had thereupon written to him his first letter, he could say to Oldenburg, whom he thereupon visited in London; "you see Newton himself has written to me; so "now you can tell me all and just a little more about it;" and then Oldenburg may have given him, and has given him, as Biot and Lefort tell us, the Analysis of Newton, but only to make extracts.

We have been obliged to elucidate this transaction, it however may have taken place in many other ways; in any case it lies before us fearfully as a naïvely told fact, that Leibnitz has secretly read the Analysis of Newton, for he made extracts from it.

Every one must immediately feel that he can only have made those extracts in London, that is, when he for the second time, we do not know for what reason, went there, being bound to Hanover; even the date is of no primary consequence; all that matters is the secrecy with which Leibnitz held possession of these extracts, for as that Analysis of Newton's was to be had printed as early as 1711 in Jones's edition, and 1712 in the *Comm. Epist.*, Leibnitz's extracts of it as of a Newtonian "manuscript" must certainly bear date at least before 1711; and now let us ask if in Leibnitz's confidential correspondence with Bernoulli, which lasts to 1712, Leibnitz had not on every page occasion to say that he had made extracts form Newton's important paper, which furnished the key to all Newton's publications, to the *Principia*, the *Memoirs upon Light*, the *Lineæ Tert. Ord.*, and the *Quadratures*. But Leibnitz did not so act; he concealed these Newtonian extracts from Bernoulli and from all his other friends, from Tschirnhaus and from all the world,—and from Newton—and yet he had here everything; for if he excerpted the analysis,

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abconabor solvere, ac tibi communicare, quamprimum otii sat erit. Just like the Chemists, of whom Kant and Boerhaave speak, who boast of discoveries which they are not yet possessed of! And what sort of a discovery was this? It is one which Leibnitz never again recurs to.

he surely did not neglect that which was best therein, even if he did not copy it, but only kept it in his memory, and extracted figures.

But as all our readers will not possess the correspondence of Bernoulli and Leibnitz in the edition of 1745, of which the index is here conclusive, we will, in order to give some conception of the frequent occasion that Leibnitz had to mention the fact to Bernoulli, if he had not been all too conscious of the necessity he was under to conceal it, copy verbatim from the index to this extensive and highly confidential correspondence (which lasted from 1694 to Leibnitz's death), the rubrics *Newtonus et Calculus infinitesimalis promotus:*

Not in one of all the above passages in this correspondence, (which lasted from 1694 later than 1712) is it stated that Leibnitz possessed anything from Newton, which the public in general did not possess. We shall not be expected to prove, that Leibnitz must have commu-

nicated with Bernoulli about these extracts of this Newtonian pamphlet, if he did not feel himself obliged to keep it secret.

We should be glad, if we had deceived ourselves in this last chapter, and if Leibnitz's extracts from the Analysis could be otherwise explained; but then the other four chapters of this Enquiry would still require no alteration.

Even if we are right in our last chapter, it need not absolutely follow that Leibnitz is a plagiarist in the worst sense; but perhaps Oldenburg did not allow him time enough; or he did not extract everything, or he saw the analysis only through Collins, and might subsequently believe that he himself was the discoverer, and that he had no need to mention what he had seen. But beyond these grounds of excuse nothing can be alleged for him.

We have been urged to this investigation by the unworthy attacks that have been made on Newton, whom we were accustomed to revere as one of our nation; and we are ready to apologize to Leibnitz, if, in the last chapter, but it is only the last we refer to, we have spoken too immoderately against him or against Oldenburg.

ADDITIONS AND REMARKS.

GERHARDT'S WORDS RESPECTING THE (LEIBNITZ) NEWTONIAN MANUSCRIPT, IN GERHARDT MATHEM. SCHRIFTEN LEIBNITZENS I. P. 7.

"I have found in the collection "of Leibnitz's manuscripts in the "library in Hannover a manuscript "with the heading: Excerpta ex "tractatu Newtoni Msco. de Ana- "lysi per aequationes numero ter- "minorum infinitas, without, as I "am sorry to say, the date at which "Leibnitz wrote the same. The "first line of this manuscript reads "as follows: $AB \sqcap x$; $BD \sqcap y$; "a, b, c, quantitates datae; m, n "numeri integri. Si $ax^{\frac{m}{n}} \sqcap y$, erit "$\frac{na}{m+n} x^{\frac{m+n}{n}} \sqcap [\int y]$ areae*. Further

* "In his Excerpta it was Leibnitz's "fashion to include his remarks as here "in parentheses.—Gerh."

"*Wir haben in der Sammlung "der Handschriften Leibnitzens auf "der Königlichen Bibliothek zu Han- "nover ein Manuscript gefunden mit "der Aufschrift: Excerpta ex trac- "tatu Neutoni Msco. de Analysi per "aequationes numero terminorum in- "finitas, auf dem leider der Vermerk "der Zeit fehlt, in welcher Leibnitz "es schrieb. Die erste Zeile dieses "Manuscripts lautet:* $AB \sqcap x$; "$BD \sqcap y$; *a, b, c quantitates datae;* "*m, n numeri integri. Si* $ax^{\frac{m}{n}} \sqcap y$; "*erit* $\frac{na}{m+n} x^{\frac{m+n}{n}} \sqcap [\int y]$ *areae*. Im*

* "In seinen Excerpten pflegte Leibnitz "die eigenen Bemerkungen durch Klam- "mern einzuschliessen.—Gerh."

"Leibnitz has only noted down the "example $\frac{1}{x^2}=y$, and the develop- "ment of $\frac{1}{1+x^2}$ in a series, and the "Newtonian Extraction of Roots; "but the chapter De Resolutione "aequationum affectarum, in which "Leibnitz seems to have interested "himself particularly, is almost com- "pletely written out."

"*Folgenden hat sich Leibnitz nur das* "*Beispiel* $\frac{1}{x^2}=y$, *ferner die Ent-* "*wickelung von* $\frac{1}{1+x^2}$ *in eine Reihe* "*und die Wurzelausziehung Newton's* "*angemerkt; dagegen ist fast voll-* "*ständig der Abschnitt: De Reso-* "*lutione aequationum affectarum,* "*ausgeschrieben, für welchen Leibnitz* "*sich besonders interessirt zu haben* "*scheint.*"

It is easy to prove that the attacks of Biot on Newton's character are unfounded. With regard to the vehement controversy between Leibnitz and Newton, Biot forgets that it did not originate with Newton. For if we were even to assume with M. Biot, that Leibnitz had more right to the discovery than he really has, still it is at all events *he* that had got *something* from Newton, and not Newton from him; for which reason Leibnitz in publishing ought to have said, that it was known to him that Newton had the very same thing which he (Leibnitz) published in 1684. Newton did not take offence at Leibnitz's thus ignoring his equal claim to the discovery; but only in his next publication, in the Principia, of which the printing was completed in 1687, added the well known Scholiam: *In literis quae mihi cum Geometra peritissimo G. G. Leibnitio annis abhinc decem intercedebant, cum significarem me compotem esse methodi determinandi maxima et minima, ducendi Tangentes, et similia peragendi, quae in terminis surdis aeque ac in rationalibus procederet, et literis transpositis hanc sententiam involventibus eamdem celarem; Rescripsit Vir Clarissimus se quoque in ejusmodi methodum incidisse, et methodum suam communicavit a mea vix abludentem; praeterquam in verborum et notarum formulis. Utriusque fundamentum continetur in hoc Lemmate.* M. Biot justly remarks in the Journal des Savants (1855, p. 603) that Newton hereby "recognises the independence of the rights of Leibnitz," "*reconnait* "*l'independance des droits de Leibnitz*," whence indeed twenty years later, in 1711, when Newton was no longer amicably disposed to Leibnitz, he found it inconvenient that there should exist a scholium so favourable to the latter, and implying such a recognition of his claims; "*en* 1711 *lorsque* "*Newton était exasperé*," says M. Biot, "*Ce scholie devenait pour lui* "*une pièce à décharge fort embarrassante:*" "in 1711, when Newton

"was exasperated, this Scholium was for him a very embarrassing "piece."

But how did Leibnitz act? He ought, as we see, to have named Newton in his publication of 1684, and the latter might have taken amiss this ignoring of his claim; still Newton mentioned in his first publication, by the side of his own, the contemporaneous right of Leibnitz. Ought not Leibnitz now at least to have confirmed this, or at all events done something rather than commence an attack upon Newton? But in truth he just now with a premeditated design injured Newton, by inserting just after Newton's Principia had been published a memoir in the *Acta Erud.* 1689, in which he, under the pretence of being as yet unacquainted with Newton's Principia of 1687, gave the most important propositions thereof on his own part. We will not dwell upon this affair; let it suffice that Biot strongly and sharply censures Leibnitz for it; and that even in their extraordinary edition of the *Commercium Epistolicum*, (1856), Messrs. Biot and Lefort repeat the censure, saying at p. 209, *Cette publication*, (*dans les Actes de Leipsig, Mens. Feb.* 1689) *est à mes yeux le seul tort que Leibnitz ait eu envers Newton, jusqu' au moment de la déplorable controverse qui a empoisonné leurs derniers jours.* So this, according to our good French friends, is (*le tort*) the wrong of Leibnitz! but herewith the affair began, for though they would gladly mystify us by using the politic phrase, *c'est á mes yeux le seul tort que Leibnitz ait eu envers Newton jusqu'au &c.*, yet no one will be so far misled by this as to forget the dates, which prove that in this *only wrong* Leibnitz committed also the *first wrong*, for before 1689 all that had been said or printed by Newton, or the latter's friends, was in no way calculated to irritate Leibnitz, but on the contrary purely amicable.

How must it have wounded Newton, that after the gigantic work of the Principia, on which he had laboured so much, and which he published in 1687, Leibnitz with French levity took the credit of the whole work to himself, publishing the interesting theorems of it on his part in 1689, as if he had at that time not seen the Principia?

It is justly said in the *Epistola ad Amicum* that if Leibnitz had really not seen the Principia two years after its publication, yet nevertheless he had seen the Epitome of the Principia which was published in the *Acta Erud.* of 1688. *Qua lecta*, says Newton, in the *Epistola ad Amicum*, *Ds. Leibnitzius schediasmata sua de motuum coelestium causis — composuit et in Actis Lipsicis ineunte anno* 1689 *imprimi curavit, quasi Ipse quoque praecipuas Newtoni de his rebus Propositiones invenisset idque methodo diversa, et librum Newtoni nondum vidisset. Qua licentia concessa Authores quilibet inventis suis facile privari possunt. Quam primum Liber Newtoni lucem vidit exemplar ejus D. Nicolao Fatio datum est ut ad Leibnitium mitteretur* (1687). *Viderat Leibnitius* (1688) *Epitomen ejus in Actis Lipsicis. Per commercium epistolicum quod cum viris doctis passim habebat, cognoscere potuit Propositiones principales in libro illo contentas imo et librum ipsum procurare. Sin Librum ipsum non vidisset, videre tamen debuisset antequam sua de iisdem rebus cogitata publicaret, idque ne festinando erraret in subjecto novo ac difficili et Newtono injurius esset auferendo inventa ejus, et Lectori molestus repetendo quae Newtonus antea dixerat.*

We repeat that it is Biot, who most severely censures Leibnitz's conduct; for he says (Article on Leibnitz in the *Biographie universelle* and Biot's *Com. Epist. Collinsii*, p. 209.) *Ainsi l'immortel ouvrage des Principes avait paru depuis deux ans et Leibnitz ne l'avait pas regardé: il ne l'avait pas regardé même après que les découvertes inouïes qu'il offrait pour la première fois au monde, avaient été annoncées dans les Actes auxquels Leibnitz renvoie; et il assure n'en avoir jamais eu connaissance que par cet extrait. Sans doute il faut le croire, car il serait trop désespérant pour l'honneur de l'esprit humain de supposer un si grand génie capable de la plus vile imposture**: *mais*

* In the Journal des Savants of 1852, p. 136, Biot adds: "*quoique d'après l'ordre des problèmes que Leibnitz attaque et d'après l'usage qu'il fait des lois de Kepler pour établir ses déductions il est à peine croyable qu'il n'ait pas daigné se renseigner plus sûrement.*" Thus he (Biot) does not believe from internal evidence (à peine) that Leibnitz had not also read the Principia.

alors il faut blâmer un dédain si aveugle ou une si condamnable insouciance.

When then for ever so long a time not a word about Newton's contemporaneous discovery of the Differential Calculus was to be wrung from the mouth of Leibnitz, and he was gradually acquiring the reputation of being the *only* discoverer, Newton's friends began to vindicate his right; and when Leibnitz not only went on, without Newton's having spoken, to state the case for himself, but also in the *Act. Erud.* of 1705, referring to Newton's publication of 1704, *de quadraturâ curvarum et de lineis tert. ord.*, came out in quite an irritating manner against Newton with the celebrated words *semper adhibuit*, for which Leibnitz was never able to justify himself, and with the comparison of Newton to Honoratus Fabrius, and of himself to Cavalleri, by which he got up the semblance of a charge of plagiarism against Newton—then the cup was filled, and Newton, who, not having as yet in the publication of 1704 said a word against Leibnitz, now finding himself with insidious phrases set down as a plagiarist, was at length—no man could have commanded himself longer—provoked to publish the documents disputing the latter's claim to the exclusive discovery.

We will give the leading passage out of this politic memoir of Leibnitz's. Leibnitz says: *Ingeniosissimus deinde Autor antequam ad Quadraturas curvarum vel potius Curvilinearum veniat, praemittit brevem Isagogen. Quae ut melius intelligatur, sciendum est cum magnitudo aliqua continue crescit, veluti Linea (exempli gratia) crescit fluxu Puncti quod eam describit, incrementa illa momentanea appellari differentias, nempe inter magnitudinem quae antea erat, et quae per mutationem momentaneam est producta; atque hinc natum esse Calculum Differentialem, eique reciprocum Summatorium; cujus elementa ab inventore D. Godefrido Guilielmo Leibnitio in his Actis sunt tradita, variique usus tum ab ipso, tum a D. D. Fratribus Bernoulliis, tum et D. Marchione Hospitalio, (cujus nuper extincti immaturam mortem omnes magnopere dolere debent, qui profundioris doctrinae profectum amant) sunt ostensi. Pro differentiis*

igitur Leibnitianis D. Newtonus adhibet, semperque adhibuit, Fluxiones, quae sunt quam proxime ut Fluentium augmenta aequalibus temporis particulis quam minimis genita; iisque tum in suis Principiis Naturae Mathematicis, tum in aliis postea editis eleganter est usus, quemadmodum et Honoratus Fabrius in sua Synopsi Geometrica, motuum progressus Cavallerianae methodo substituit.

We see that Leibnitz had not really the courage to say openly, that he was the first, and Newton the second discoverer, but that he only gave a glimpse of this intimation by the concluding word *substituit*, and through the word *adhibet*, to which he mysteriously appended the words *semperque adhibuit*; moreover Leibnitz had not even the courage to sign his name to the article, but denied stubbornly, even till his death, that he had written this Review; which however every one looked upon as having proceeded from him, and which, as is now proved, he had really written.

How long then, in order to satisfy M. Biot, ought Newton and his friends to have been silent? So far was the *Commercium Epistolicum* from being an act of aggression, that we have much rather reason to say, that it would have been no longer fair to fight a disguised battle, and with politic phrases to cover the case thinly over, while the documents could be brought forward.

Thus it cannot be maintained that Newton was the irreconcileable strife-loving party; Newton was mild and loved tranquillity; he even abstained from editing Kinkhuysen's Algebra, *ne quietem suam perderet*, (Com. Ep. No. 23, and 57), and he wrote to Leibnitz even in 1693, *Spero me nihil scripsisse, quod tibi non placeat, aut si quid sit, ut literis id mihi significes, quoniam amicos pluris facio quam inventa mathematica.*

The individual small attacks of Biot on Newton have as little foundation. *Il ne faut pas*, says Biot in 1832, (*Jour. des Sav.* p. 271), *vouloir justifier Newton d'avoir supprimé dans la troisième Edition des Principes le célèbre scholie qu'il avait inseré dans les premierès et qui reconnaissait les droits de Leibnitz.* That is, M. Biot wants Newton, after he has engaged in a dispute with Leibnitz, to acknowledge once

more that which he now denied; a singular demand indeed! It is here again to be remarked in Newton's favour, that he simply withdrew the scholium favourable to Leibnitz, without replacing it by a hostile one. *Enfin*, continues M. Biot, *il ne faut pas trouver beau, ni juste ni honorable, à Newton d'avoir encore poursuivi son rival dans la tombe, par une nouvelle édition du Commercium Epistolicum augmentée de deux nouvelles lettres de Leibnitz, qu'il s'etait procurées, et qu'il accompagna d'une réfutation très amère.* The Comm. Ep. was made public during the lifetime of Leibnitz, and the *lettres que Newton s'était procurées* are by no means private letters, but one of them is the *charta volans mathematica* 7 *Julii* 1713, which Newton had certainly no need to get, as Biot's words would lead us to imagine, by any unallowable means, because Leibnitz himself had everywhere circulated it, in order thereby to assail Newton; and the other is the reply of Newton's friends thereunto, (the "refutation" being the Ad lectorem of this edition: for the Recensio reprinted there, had appeared in the Philosophical Transactions one year and eight months, and in the Journal Litéraire, in French, one year and seven months before the death of Leibnitz).*

M. Biot further makes mention of an account-book of the year 1659, kept by Newton (who was at that time sixteen years old) which has been brought to light, of which Sir David Brewster (Biot's authority) thus speaks:

"*At the end of the book there is a list of his expenses, entitled Impensa* "*propria, occupying fourteen pages. On the 4th page the expenses are* "*summed up thus:—*

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* Even Newton's "Remarks" on the last Leibnitian pleadings before Conti (of 9th April, 1716) were written before Leibnitz's death, that is, before the 14th Nov., 1716. It is not Des Maizeaux's collection of 1720, in which (see pages 78, 98) those Remarks dated 18th May, 1716, were first printed, but the History of Fluxious by Raphson, of which book pages 97—123 are as it were a second edition.

Totum	£3 5 6	
Habui	4 0 0	
Habeo	0 14 6	

"*On the 5th page there are fourteen loans of money, extended thus:*

Lent Agatha . .	£0 11 1
Lent Gooch . .	1 0 0

"*and he then adds at the bottom of the page, lent out 13 shillings more* "*than £4.*

"*Among the entries are Chessemen and dial* . £0 1 4

Effigies amoris . . .	0 1 0
Do	0 0 10

"*and on the last page are entered seven loans, amounting to £3. 2s. 6d.* "*There is likewise an entry of 'Income from a glasse and other things to* "*'my chamber-fellow, £0 0 9.' Another page is entitled*

Otiose et frustra expensa.

Supersedeas.	*Sherbet and reaskes.*
China ale.	*Beere.*
Cherries.	*Cake.*
Tart.	*Bread.*
Bottled beer.	*Milk.*
Marmelot.	*Butter.*
Custards.	*Cheese."*

M. Biot is very facetious on the subject of such details being offered to the public in England, but he might be facetious against himself for he adds still more detail, saying: *Ne voulant pas imiter le singe de la fable qui prenait le Pirée pour un nom d'homme, j'ai eu recours à l'obligeance de M. le professeur De Morgan, le priant de vouloir bien m'interpréter les mots dont le sens me semblait douteux, ou qui m'étaient tout à fait inintelligibles. Grace à lui, je vais ici me prévaloir de son érudition archéologique dans la langue de Cambridge, en faveur des lecteurs français, peut-être même anglais qui voudraient connaître au juste, en quoi consistaient les excès de Newton.*

Marmelot équivaut évidemment au mot actuel marmalade, en français

marmelade; Reaskes, maintenant Ruskes, désigne une sorte de biscuits légers.

China ale, littéralement l'ale de Chine. Tout le monde sait que l'ale est une sorte de bière légère de couleur jaune pâle. Mais qu'est-ce que l'ale de Chine? M. de Morgan a ingénieusemsnt deviné que ce devait être là une locution employée alors parmi les étudiants de Cambridge pour désigner le thé. We see that M. Biot and M. de Morgan have made some exertions in order to be able to impart those details to the public with still more precision than they were given with before them.

We quote from the same article of Biot's, in 1855, the following passage about Newton's character: *Aux occasions rares où il lui arrivait (Newton) d'assister à des banquets publics dans la salle commune du collége, si l'on n'avait pas la précaution de l'y faire penser, il arrivait en désordre, les souliers abattus sur les talons, les bas non attachés, les cheveux non peignés, et un surplis sur le tout. D'autres fois il sortait le long d'une rue sans songer qu'il n'était pas convenablement habillé; puis s'en apercevant il regagnait bien vite son logis tout honteux. D'auditeurs il n'en avait que très-peu ou pas du tout, et il faisait le plus souvent ses leçons devant les murailles. On ne le voyait jamais non plus prendre aucun amusement, aucun exercice, se mêler à aucun jeu. Il se délassait d'une étude par une autre, toujours pensant, toujours méditant. Il était rare qu'il se couchât avant deux heures du matin, pour se lever vers cinq ou six; dormant au plus quatre ou cinq heures. Quant à son caractère moral dans le peu de commerce qu'il avait avec le reste des hommes, on le représente doux, posé, inoffensif, ne se mettant jamais en colère; de plus charitable et généreux dans l'occasion. Ces derniers penchants, on sait qu'il les garda toujours, et l'accroissement de sa fortune ne fit que lui donner les moyens de s'y abandonner plus librement.*

That M. Biot cannot understand such a character we perfectly comprehend, yet this is not the fault of Newton, but of Biot himself.

Altogether Newton as inventor is in his right, even by the established conventions of the literary world, according to which an act of publication is necessary to establish one's title in a discovery, because mere thoughts may have been by the thinker himself considered valueless. For Newton in 1669 sent his Differential Calculus to the President of the Royal Society in London; he had given it previously to Barrow; the Secretary of the Society Oldenburg and Collins had also copies. Newton had not forbidden these persons to speak of it, and Collins as well as Oldenburg made this discovery everywhere known as early as the year 1669, as is evinced by letters in the *Comm. Epist.*, from Oldenburg, 14th September, 1669, to Sluse in Leyden; from Collins, 25th November, 1669, to James Gregory; from Collins, December, 1671, to Borelli in London; from the same, 26th December, 1671, to Vernon; from Oldenburg in several communications to Leibnitz before 1675; but Collins and Oldenburg of course did not come forward with the detail, because it was proper that this should proceed from Newton, who meant to publish it in Kinkhuysen's Algebra, but was prevented by the controversies which arose from the publication on his Theory of Colours, and the labours which he bestowed upon the Principia. Newton thus made an exception to the practice of the Geometers of his time, because he did not keep to himself the method that he had discovered, but had written it down completely, and without reserve, and at once put it in circulation amongst his friends.

It is commonly thought, that Newton eagerly concealed that which he had discovered, while Leibnitz, more magnanimous than Newton, communicated to the world all he knew, but Leibnitz cannot even claim the honour of this communicativeness; for his publication in

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the *Acta Eruditorum* in 1684 was not made to be understood, but to be not understood. The publication was even not understood by mathematicians, such as Bernoulli—and James Bernoulli requested Leibnitz to expound to him that which was unintelligible therein. And Leibnitz did not answer.

In the *Mémoires de l'Académie* of 1705 we read: *Mr. Jac. Bernoulli pénétroit déjà dans la Géometrie la plus abstruse, et la perfectionnoit par ses découvertes, à mesure qu'il l'étudioit, lorsqu'en* 1684 *la face de la Géometrie changea presque tout à coup. L'illustre M. Leibnits donna dans les Actes de Leipsic quelques essais de son nouveau Calcul differentiel, ou des Infiniment petits, dont il cachoit l'art et la methode. Aussi-tôt Mrs. Bernoulli, car M. Bernoulli l'un de ses frères, et son cadet, fameux Géometre, a la même part à cette gloire, sentirent par le peu qu'ils voyoient de ce calcul quelle en devoit être l'étendue et la beauté, ils s'appliquèrent opiniatrément à en chercher le secret, et l'enlever à l'inventeur, ils y rèussirent, et perfectionnèrent cette Methode au point que M. Leibnits par une sincerité digne d'un grand homme a déclaré qu'elle leur appartenoit autant qu'à lui.*

Gerhardt endorses the above assertion, and we do so with him. He namely tells us, Leib. Math. Works, III, p. 5, 1855. "In the "*Acta Erud.*, in 1684, Leibnitz had made known his new method; "James Bernoulli could readily imagine of what importance it was; "yet he was unable to raise the veil which, as it appeared, concealed "almost impenetrably the very concisely enunciated principle. At "last in the year 1687 an opportunity presented itself to James "Bernoulli to enter upon a correspondence with Leibnitz, the author "of the new method, and request him to furnish explanations and "directions by which to understand it. This letter of Bernoulli's was "delivered, while Leibnitz was absent upon a long journey——; and "so it happened that Leibnitz did not furnish James Bernoulli with "an answer to it, till after his return in the year 1690, when however "he had no longer any need to instruct Bernoulli in the principle of "this higher analysis. For Bernoulli had, *proprio Marte*, and by a

"persevering study, penetrated the mystery, and had already manifested "the proficiency he had acquired by the solution of the isochronous-"curve-problem, which Leibnitz had proposed to the Cartesians." "In den *Actis erud.* hatte Leibnitz 1684 seine neue Methode bekannt "gemacht; Jac. Bernoulli mochte wohl ahnen, von welcher Wichtigkeit "sie sein könnte; dennoch vermochte er den Schleier nicht zu lüften, "der das in grösster Kürze dargestellte Princip derselben, wie es schien, "fast undurchdringlich verhüllte. Endlich im Jahre 1687 bot sich Jac. "Bernoulli eine Gelegenheit dar, mit Leibnitz selbst, dem Verf. jener "neuen Methode, eine Correspondenz anzuknüpfen und ihn um die "Aufklärung und Anleitung zum Verständniss zu bitten. Dieses Sch-"reiben von Bernoulli traf —— indess ein, als Leibnitz auf einer grossen "Reise begriffen ——. So geschah es, dass Leibnitz erst nach seiner "Rückkehr im Jahre 1690 eine Antwort darauf an Jac. Bernoulli "übersandte, in der er jedoch letzteren nicht mehr über das Princip der "höhern Analysis zu belehren nöthig hatte. Denn derselbe war durch "eigne Kraft und durch ein beharrliches Studium in das Mysterium "eingedrungen und hatte bereits seine erlangte Meisterschaft durch die "Lösung des von Leibnitz den Cartesianern vorgelegten Problems der "isochronischen Curve bekundet."

Thus far Gerhardt, the warm friend of Leibnitz. It is therefore entirely incorrect to say, that Leibnitz openly published the Differential Calculus. The contrary is manifest. Bernoulli had to find it out by his own ability; in doing which he was certainly assisted by Leibnitz's notice of 1684 and 1686, but perhaps not less by the publication of Newton's Principia in 1687. Such is the case of the "publication." And it is Gerhardt himself who cannot help admitting this against Leibnitz.

It is well known that the Marquis de l'Hôpital was nominally the publisher of the detail of the Leibnitzian Differential Calculus, and it is a matter of course that he over and over again names Leibnitz as the discoverer of this method. It is however this very man's statement which makes against Leibnitz; Newton has remarked this at the end of the Recensio in the words: *Nondum, inquit Hospitalius, tam simplex erat (Tangentium methodus) quam a Barrovio reddita est, naturam Polygonorum propius considerando, quod sponte menti objicit parvulum Triangulum, compositum ex particula Curvae inter duas ordinatas sibi infinite propinquas jacentis, et ex differentia duarum istarum Ordinatarum, duarumque itidem correspondentium Abscissarum. Atque hoc Triangulum illi simile est, quod ex Tangente et Ordinata et Subtangente fieri debet: adeo ut per unam simplicem Analogiam omnis jam Calculatio evitetur, quae et in Cartesiana et in hac ipsa prius Methodo necessaria erat. Quo tamen vel haec vel Cartesiana revocari ad usus posset, necessario tollendae erant Fractiones et Radicales. Ob huius itaque Calculi imperfectionem, introductus est ille alter Celeberrimi Leibnitii, qui insignis Geometra inde est exorsus, ubi Barrovius aliique desierant. Porro hic ejus Calculus in Regiones hactenus ignotas aditum fecit; atque ibi tot et tanta patefecit, quae vel doctissimos totius Europae Mathematicos in admirationem conjecerunt, etc.*

Hactenus Hospitalius. Non viderat nimirum Newtoni Analysin, neque Epistolas ejus 10 *Dec.* 1672, 13 *Jun.* 1676 *et* 23 *Oct.* 1676 *datas: quarum nulla ante annum* 1699 *typis publicata est: nescius itaque Newtonum haec omnia effecisse atque indicasse Leibnitio, Leibnitium ipsum arbitratus est inde incepisse ubi desierat Barrovius.*

Instead of naming the Marquis de l'Hôpital as the author of the *Analyse des infiniment petits*, published in 1696, it is at last time that

we should attribute this important book to its true author John Bernoulli, though l'Hôpital represents himself to be its author: In Bernoulli's opera IV. p. 387—558, we read: *Johannis Bernoullii Lectiones mathematicae de methodo integralium aliisque conscriptae in usum Illi March. Hospitalii cum auctor Parisiis ageret Annis* 1691 *et* 1692 *Lectio prima: De natura et Calculo integralium. Vidimus in praecedentibus* (*Intelligit*, says the note, *Lectiones in calculum differentialem quae praecesserunt, quasque supprimendas duxit, siquidem omnia quae in lectionibus istis continentur ab Hospitalio relata fuerunt in librum suum quem inscripsit Analyse des infiniment petits*).

Upon this same subject John Bernoulli himself writes to Leibnitz, (compare the edition of Gerhardt, p. 480; for all the other editions do not contain this passage;) *De suo aliud nihil addidit* (*Hospitalius*) *nisi quod tres quatuorve paginas repleat. Sed nolim quicquam ipsi de hisce referas, aliàs qui jam amicissimus mihi est, eum haud dubie infensissimum haberem.* It was thus that Bernoulli published the Differential Calculus, and allowed a rich French Marquis to designate himself as the author of the publication, 1696.

The conclusion that the Recensio is the work of Newton, is one that De Morgan was not the first to arrive at, as M. Biot makes out in compliment to his fellow-labourer Mr. De Morgan, who simply repeated this from a positive statement of Wilson's and only added some "internal evidence," saying: "*throughout the whole* (*of the Recensio*) "*there is not one compliment to Newton* (*except in quotations introduced* "*in proof of assertions*) *not one word expressive of admiration, and not* "*one reference to any thing he had done which he might not in perfect good* "*taste have been the author of. Who could have written thus about* "*Newton, except Newton himself.*"

It was not then uncommon to write anonymously as Newton has done in the Recensio; the practice was not merely innocuous, but so far useful, as the matter of the work was thus left to speak for itself. Also Leibnitz often wrote anonymously in the *Acta Erud.* and other places. His biographer Guhrauer says somewhere, Observations on page 186, V. II: "Dass diese Schrift aus Leibnitzens eigner Feder geflossen, "lehrt Inhalt und Schreibart: das Leibnitz darin beigelegte Lob bildet "keinen Einwand; er war in solchen Dingen ganz objectiv." "That "this piece came directly from the pen of Leibnitz, is told by the "style alike and the import; the praise therein bestowed upon "Leibnitz constitutes no objection to this view; he was in such things "altogether objective." We see that Newton in writing anonymously was habitually more modest and *subjective*, in the opinion not of his biographer, but of his eager opponent.

The new edition of the *Commercium Epist.*, which appeared in France, cannot be said to possess much value. Of the variations of the first from the second edition, there was but one, to which Professor De Morgan himself, who has discovered their existence, could attribute the smallest consequence, (and since 1848 now that the question about the letter of the 10th December 1672 depends no more upon the silence of Leibnitz, even this variation is no longer worth any attention); all the others have never been of the slightest importance. What are we then to understand, when the French edition makes these petty variations the pretence for its publication? Why do not Messrs. Biot and Lefort tell us, that which was remarked here by that very Professor De Morgan to whom they and we are indebted for these various readings? " *Those who are acquainted with* " *the bibliographical habits of the beginning of the last century will not* " *impute wilful unfairness even to such additions and suppressions as* " *some of those I shall have to describe.*" These are De Morgan's own words, and it is readily comprehended that the very number of the additions makes them so easy of detection, and therefore a disingenuous intention about them quite impossible. The French edition complains naïvely, that all these additions are not favourable to Leibnitz. Messrs. Biot and Lefort should not have found this surprising, for indeed the whole *Commercium Epistolicum* is unfavourable to Leibnitz; but there is an inexactness even in their statement which has been already previously acknowledged by Professor De Morgan, who has remarked that the non-mention in the first edition of the year of Collins's death was more serviceable for an attack upon Leibnitz than the citation of this date in the second edition. The French editors of the *Comm. Ep.* in 1856, Messrs. Biot and Lefort, misunderstand moreover what is

written in the German language and so give for instance as a postscript to Bernoulli, what was never a postscript, (Gerh. Works of Leibnitz, Pt. 3, p. 66 to 73, and Biot and Lefort, loc. cit., p. 266) which has a very comic effect, for in Biot and Lefort's edition the letter now runs: *Ceterum an eam mihi animi parvitatem tribuis, ut tibi vel fratri tuo succenseam, si quos in Barrovio usus perspexistis quos mihi, inventionum contemporaneo, ab eo petere necesse non fuit;* and the Postscript runs: *P. P. An eam in me animi parvitatem putas, ut vel tibi, vel D. fratri tuo, succenseam, si vos in Barrovio usus perspexistis, quos mihi, inventionum contemporaneo, ab eo petere necesse non fuit.* M. Lefort accordingly believes that Leibnitz repeated his letter in his postscript. We know whence this proceeds. M. Lefort did not understand the two German lines, which Gerhardt introduced at p. 71. This new edition would have been somewhat useful, if it had furnished the correspondence of Leibnitz with Newton after the first edition of the *Commercium* after 1712, but though this is promised in the Table of Contents, the text gives, in lieu thereof, merely little politic abstracts of these later letters, and there is nothing about Gerhardt's Tracts, because unfortunately the French editors do not read anything which is German.*

* It may be also presumed that the principal editor, M. Lefort, does not understand English, for otherwise M. Biot would not have signed the single citation at p. 45 with the initials J. B. B., while all such other citations are signed Lefort. M. Lefort says, in a particular observation at page 248: *Je ne me crois pas obligé de suivre l'orthographe de l'ouvrage de sir D. Brewster. Quand on voit écrit, par exemple tome II. p. 429 cognitam pour cognatam et p. 435 très-semble pour très-humble, on peut craindre que les épreuves n'aient pas été revues par une personne assez familière, avec les langues latine et française.* M. Lefort as we see piques himself upon his knowledge of his native French language. But if he is so strict with the orthography of the two languages, Latin and French, of which the latter may in some degree be known to him, we remark that the person to whom he entrusted the correction of the only five German words that appear in his edition, should, in the word *Schriften*, p. 287, have made the first letter a capital, because we have here a noun-substantive, like the others, in which this person has employed a capital letter, and that M. Biot might have instructed M. Lefort, that though Sir David Brewster is here reprimanded for an orthographical error, yet through this whole reprimand it would orthographically have been correct to have written *Sir*, with a capital *S*.

We repeat that people in Germany will know better how to defend Leibnitz, which also is not the object of Messrs. Biot and Lefort. Fermat's name must positively not be forgotten; it is on this account that the French will have their say in this controversy; on this account clear water must be muddled; on this account they put themselves on the weaker side, because it would look too extraordinary, if France were to designate Newton as the sole inventor, and slip in Fermat. At this people would smile still more, as also now they smile; for whatever is done, no one relishes that Fermat sauce. The controversy about the discovery of the Differential Calculus is a question between England and Germany, from which the French must keep their finger away; let them come in honourably, if they like it, as judges; but if they want to make an independent party in the contest, we must shut them out, and fight by ourselves; the weaker party even disdains such equivocal succours. What Leibnitz did for the Differential Calculus, even if he did not discover it, is at any rate infinitely more than Fermat has done, as, indeed, no Frenchman before the middle of the succeeding century, achieved anything at all therein; for all that L'Hôpital, the rich and influential, appropriated to himself, he had in reality taken from Bernoulli, who was silent because his Lectiones were splendidly paid for by the Marquis. As L'Hôpital and Bernoulli, 1696, did not know what the *Comm. Epistolicum* communicated to them, so in 1712, the *Comm. Epist.* and Newton himself were ignorant of what Gerhardt has communicated to us, viz. that Leibnitz had perused Newton's Analysis. Where would this question now be, if Newton had been able to lay before the public the clandestine Leibnitzian excerpta without date out of his Newton's Analysis? Then would Bernoulli, the honest Bernoulli, whom Leibnitz betrayed, have been unable to strive in his behalf, and Leibnitz himself would have been obliged to couch his lance. And while this is the question, Messrs. Biot and Lefort go collecting little various readings of a book, which being but too moderate did not once intimate what Newton scarcely suspected. This edition of

M 2

Messrs. Biot and Lefort's is indeed everywhere a singular one: for instance at p. 196, M. Lefort has revealed to us that Leibnitz had found the exact quadrature of the circle. Every one who reads what is there quoted, will understand us. On page 199 M. Lefort says: *En rétablissant encore (!) ici un paragraphe omis ou tronqué, j'ai voulu montrer l'esprit qui a présidé aux extraits du Commercium Epistolicum (de* 1712) *et réduire à sa juste valeur le certificat d'impartialité délivré par l'Abbé Conti aux éditeurs.* Here that which is evident is only the malice of M. Lefort. He himself, or anybody else would have left out what is here missing, because it speaks irrelevantly about the solutions of equations, as irrelevantly as if it had spoken about M. Lefort. At page 204 Biot and Lefort say that Newton's second letter of 24th October was nine months in reaching Leibnitz, "*par suite de ses nombreux voyages,*" "in consequence of his numerous travels." Leibnitz merely travelled from London passing through Holland to Hanover, in not quite two months, for he arrived (Gerhardt I. p. 27) at his destination (Hannover) in December. The words of Oldenburg (in Gerhardt's Math. Works of Leibnitz, I. p. 151,) *dites donc, s'il vous plaît, si je dois bailler la grande lettre de Newton*, and the suspicious word *Hodie* (Gerh. ibidem, p. 154, cf. Comm. Ep. of 1712, No. LXVI.) even gives us reason to apprehend that Leibnitz had already read the letter of 24th October in London, and that it was but officially that afterwards, nine months after it was written and five months after his arrival in Hanover, he had it sent to him once more.

At p. 285, M. Lefort says: *Si la publication du Commercium Epistolicum en* 1712 *fut une oeuvre de parti, que dire de sa réimpression en* 1722, *six ans après la mort de Leibnitz? Dans cette prétendue réimpression, le nouvel éditeur corrige, ajoute, retranche, interpole, commente; et la passion l'aveugle au point qu'il écrit, sans l'y voir, sa propre condamnation dans l'étonnante pièce de polémique qui résume le livre auquel elle sert de préface.*

So says M. Lefort without further additions. We leave to the reader

the satisfaction of discovering for himself, what *pièce ètonnante* this is, in which Newton has done so much towards his own prejudice, for M. Lefort has clearly enough designated the *pièce*, but the incredibility of his verdict forces one to be a long time in search of what he has meant.

M. Gerhardt is completely thrown out of his saddle, by his supposition that when in a paper Leibnitz's writes *S pro omne* he thereby invents the "Integral" calculus, and that this had preceded the later invention of his Differential Calculus. Thus M. Gerhardt gets quite into an ill-humour with John Bernoulli, and says of him, "that he "has all his veins filled with unmeasured and extravagant pride and pretensions" "er strotzt durch und durch von ungemessenem Stolz "und höchster Anmassung" (Leib. Works, III p. 113), adding at p. 115, "in general John Bernoulli is considered as the discoverer "of the Integral Calculus," and that Leibnitz discovered the Integral and afterwards the Differential Calculus. "But for the reason, that "as regards the Differential Calculus, he was able to exhibit general "propositions, he therefore made this publicly known and kept back "the Integral Calculus, in which he was unable to exhibit any general "method." "Allgemein hält man Joh. Bernoulli für den Entdecker "der Integralrechnung," Leibnitz habe die Integral-Rechnung und später erst die Differenzial-Rechnung entdeckt. "Aber aus dem Grunde "wahrscheinlich, dass er für die Differenzial-Rechnung allgemeine "Lehrsätze aufstellen konnte—aus diesem Grunde machte er allein die "Differenzial-Rechnung bekannt und hielt die Integral-Rechnung, für "welche er solche allgemeine Methoden nicht aufstellen konnte, zurück." We have sufficiently remarked that the first discovery of the *Integral Calculus*, as summation, did not wait for Leibnitz, inasmuch as Wallis had written a book upon the same as early as 1657. On the other hand the invention of the Integral Calculus, in the higher sense, as is commonly and justly supposed, was not only not first achieved by Leibnitz, but in this sense was only achieved by John Bernoulli. Gerhardt falls into a glaring contradiction after this violent attack upon

Bernoulli: for at p. 114, *loc. cit.*, Gerhardt says, that the first letter (from Bernoulli) to Leibnitz is full "of the most adulatory praises of "the latter;" and hence "because Leibnitz was at no time inaccessible "to such offerings," (voll "der schmeichelhaftesten Lobeserhebungen des "letztern," und daher "weil Leibnitz für dergleichen durchaus nicht "unempfänglich") this correspondence between these two became, says Gerhardt, the most voluminous of all. How does this agree with Gerhardt's just now quoted statement about Bernoulli, that "his veins "were filled with unmeasured pride and extravagant pretensions?" The fact is, that Bernoulli does not manifest either of these extremes in his character, and that Gerhardt is merely disconcerted, without exactly knowing why, but in the feeling that his Theory, of Leibnitz having first invented the Integral Calculus, and then the Differential, will in what he here has to say about Bernoulli not suit at all—an idea, according to which one should cease to designate Leibnitz as the inventor of the Differential Calculus, and since even the word Integral is an invention of Bernoulli, one would have to make Leibnitz the inventor of Wallis's summatory idea, which new view though it would at first have astounded Leibnitz, might perhaps on closer reflection have suited him just as well as it now suits Gerhardt. The only thing wanting is that M. Gerhardt should get out of temper, not with John Bernoulli alone, towards whom he is quite ill-disposed, but also against Leibnitz, because the latter supposed that he had invented something else, which does not suit M. Gerhardt. Again John Bernoulli at last, (Works of Leibnitz, III., p. 132,) is praised by Gerhardt even more than there is reason. "After his (Leibnitz's) death," we read, "the controversy" (about the discovery of the Differential Calculus; cf. p. 131, from the words *nicht offentlich*,) "was openly taken up by "John Bernoulli, and maintained triumphantly to the signal discomfiture "of the English." "Nach seinem (Leibnitzens) Tode wurde der Kampf "(über die Erfindung der Differenzial-Rechnung cf. S. 131. die Worte "'nicht öffentlich') von Seiten Joh. Bernoulli's offen aufgenommen und "siegreich mit grosser Demüthigung der Engländer geführt." Now

this again is incorrect. On the contrary John Bernoulli, after Leibnitz's death, apologized to Newton, as appears from the letter of his to Newton which is so well known and quoted also by Lefort (page 250.) In this and all his last letters (cf. Brewster, II. 504, Edleston, page 169, note) John Bernoulli courts the friendship of Newton, assuring him, that it was not true, as Leibnitz treacherously said, that he (Bernoulli) had ever written anonymously on the question against Newton, though this was true. Where have we here a controversy with the at last discomfited Newton? No! No! Newton's claims are too firmly established. "All controversy about the discovery is at an end," cries Gerhardt in behalf of Leibnitz. Gerhardt begins triumphing too soon, and this is our excuse for speaking too strongly perhaps against Leibnitz, whom clever Frenchmen extol so high, and for Newton whom Gerhardt courageously defending a German great man, could and dared not appreciate.

In order to show how clear, one might almost say how over-clear, if this were possible, a question can be made in France, when there is no deliberate intention of perplexing it, let us quote at full length Montucla's judgment (in his History of Mathematics, III. p. 109).

Il est temps, says Montucla, *de nous résumer, et d'abord on ne peut douter, que Neuton ne soit le premier inventeur des calculs dont il s'agit. Les preuves en sont plus claires que le jour; mais Leibnitz est-il coupable d'avoir publié comme sienne une découverte qu'il auroit puisée dans les écrits même de Neuton? c'est ce que nous ne pensons pas. Dans les deux lettres de Neuton, communiquées à Leibnitz, on ne voit que des résultats de la méthode ou des deux méthodes employées par Neuton; mais non leur explication. Un homme doué d'une sagacité transcendante tel qu'étoit Leibnitz, n'a-t-il pas pu être excité par là à rechercher les moyens employés par Neuton et y réussir; d'autant que Fermat, Barrow et Wallis avoient ouvert la voie. En effet si l'on considère combien peu il y avoit à faire pour passer de leurs méthodes au calcul différentiel; il paroîtra, ce semble, superflu de rechercher ailleurs l'origine de ce dernier: car ce que Barrow désignoit par e et a n'étoit que les incrémens ou décrémens simultanés de l'abscisse et de l'ordonnée, lorsqu'ils étoient devenus assez petits pour pouvoir retrancher du calcul leurs puissances supérieures à la première: or en supposant, par exemple, cette équation* $x^3 = by^2$, *le calcul de Barrow donnoit* $3x^2e = 2bya$; *de même l'équation* $x^4 = b3y$ *donnoit* $4x^3e = 3ba$. *L'analogie conduisoit donc à remarquer que si l'on avoit* $x^n = y$ *on devoit avoir* $nx^{n-1}e = a$, *quelque fût le nombre n, entier ou fractionaire, positif ou négatif, et conséquemment l'incrément, par exemple, de* $\sqrt{x}$ *ou* $x^{\frac{1}{2}}$ *devoit se trouver* $\frac{1}{2}x^{\frac{1}{2}-1}e$; *ou au lieu de e, mettant une caractéristique qui donne à reconnoître son origine, comme dx* (*c'est celle qu'a*

choisie Leibnitz) voilà l'écueil des irrationalités décliné et le passage du calcul de Fermat, Barrow et Wallis au calcul différentiel de Leibnitz, et de cette seule observation dépendent toutes les opérations de ce calcul. Ajoutons, quant au calcul inverse, que Wallis avoit déjà désigné les élémens des aires des courbes par le rectangle fait de l'ordonnée et d'une portion infiniment petite de l'abscisse qu'il nommoit A, de sorte que l'élément de l'aire du cercle étoit, par exemple, $A\sqrt{aa-xx}$. *Il avoit aussi réduit à de semblables expressions les élémens des longueurs des courbes, et même par une analogie fondée sur la ressemblance du petit triangle caractéristique avec celui de la soutangente, de la tangente et de l'ordonnée.*

It is clear Montucla does not do the same thing with Biot, (whom however, since he has united on his head the three crowns of the Academy, an honor that falls to the lot of few mortals, one must look upon as the greatest man in France)—for he counts Wallis among those *qui ont preparé l'invention au dixseptième siècle*, and we may therefore choose to let it pass that Fermat is here also named in too good company perhaps rather conspicuously.

Dutens's edition of Leibnitz's Mathematical works, (Pt. 3 of the Opera), published after Leibnitz's death 1768, is prefaced, as is but reasonable, with an eulogium upon Leibnitz by Joucourt; we nevertheless in this very shrine of Leibnitz's highest glories read not that which none would have ventured to say except the Journal des Savants, viz. that Newton had not as yet discovered all; but Joucourt, the geometer, the biographer and panegyrist of Leibnitz, says here in Leibnitz's works, Preface p. XXXIX. at the end of his history of the invention—*Newtonum fateor, pro meâ æstimatione, primum inventorem fuisse calculi differentialis;* and thus these *Opera Leibnitii* of Dutens or Joucourt, which is as much as saying Leibnitz himself, do not go so far in the praise of Leibnitz as Biot, but only so far as Montucla does.

The first letter which Leibnitz wrote to Galloys, one of those inferior geometers in Paris, at the time when he himself was there, is significative for the illustration of his doings in mathematical affairs in general. We give this letter (see Gerhardt's Edition, I., p. 177):

Une indisposition m'a empeché de faire ma cour cette semaine comme je me l'estois proposé. C'est pourquoy je Vous supplie de suppléer par vostre bonté au defaut de ma presence, si l'occasion se presente de parler utilement de l'affaire qui vous est renvoyée, est j'espere que vos faveurs seront bientost suivies d'un succès favorable.

Je n'ay pas osé écrire à Mons. le Duc de Cheureuse, de peur d'abuser de la grace qu'il me fait de ne me pas rebuter entièrement, lorsque je viens quelquesfois luy faire la reverence. Mais je sçay que Vos recommandations serviront bien mieux à me conserver l'honneur de la protection que tout ce que je pourrois écrire.

Comme je ne veux pas abuser de vostre temps, qui est dû au public, et à des personnes pour lesquelles le public s'interesse ; je ne veux adjouter que le recit d'une petite conqueste que je viens de faire sur l'Hyperbole. Tout le monde sçait qu' Archimede a donné la dimension de la Courbe du Cercle en supposant la quadrature de la figure. Messieurs Hugens, Wallis et Heuraets ont fait voir que la Courbe de la Parabole depend de la Quadrature de l'Hyperbole. Mais personne a donné encor la dimension de la Courbe de l'Hyperbole par la Quadrature de son espace ; non pas même de celle de l'Hyperbole principale, qui a les asymptotes à angle droit et les costez rectum et transversum égaux, et qui est entre les Hyperboles ce que le Cercle est entre les Ellipses. J'en suis venu à bout à la fin par un effort d'esprit sur ce que Mons. Oldenbourg m'avoit écrit dépuis peu que Messieurs les Anglois l'avoient cherchée, et la cherchoient encor sans succès. Cela m'anima à faire une petite tentative, d'autant

plus que je sçavois que Mons. Gregory (qui est grand Geometre sans doute) y avoit renoncé en quelque façon publiquement dans sa Geometrie des Courvilignes. Mais je vous en parleray plus amplement, quand j'auray l'honneur de vous saluer, cependant je me dis etc.

This letter is dated Paris, 2nd November, 1675. Oldenburg's letter (*que M. Oldenbourg m'avait écrit depuis peu,*) which Galloys indeed was not acquainted with, is known to us; it had just come fresh from England, and is dated 30th September, 1675; it runs as follows:

Oldenburg to Leibnitz:—scire cupis, an dare Nostrates Geometrice possint dimensionem Curvae Ellipseos aut Hyperbolae ex data Circuli aut Hyperbolae quadratura. Respondet Collinius, illos id praestare non posse Geometrica praecisione, sed dare eos posse ejusmodi approximationes, quae quacunque quantitate data minus a scopo aberrabunt. Et speciatim quod attinet alicujus arcus Circuli rectificationem, impertiri Tibi poterit laudatus Tschirnhausius methodum a Gregorio nostro inventam, quam, cum ille apud nos esset, Collinius ipsi communicavit. Thus it is not true, that Oldenburg had written: "*que Messieurs les Anglais le cherchaient sans* "*succès;*" for Leibnitz himself had not sent to Galloys that quadrature or rectification of the circle or hyperbola, which now remains and for ever will remain an impossibility, but only an approximation to it; but that very thing which Leibnitz pretends to have discovered, sneering at the English for not having done so, he obtained through the English, and rediscovered it after them. Even here the excuse remains, that what Leibnitz sent to Galloys, was perhaps not the same thing as he got from Tschirnhaus; but he concealed the fact that he had got something from that quarter. This was his system. "*Messieurs les Anglais,*" says Leibnitz, while on the contrary (see Gerhardt, I. p. 55,) he calls his French geometers *nostros geometras* characterizing himself as a Frenchman, as indeed he was. From *Messieurs les Anglais* Leibnitz gets his wisdom, petitioning them for that which as yet they had only in an unprinted form, and then he writes, *je n'ai pas pu vous faire la cour*, and *je viens de faire une petite conquête sur l'hyperbole.* In this *Messieurs les Anglais* the whole matter

is comprehended. Let it not be said that Tschirnhaus, to whose English mathematical documents Leibnitz was referred, perhaps kept these back—so that Leibnitz could not in this way make his *conquêts* of what the English had conquered; for of the intimacy between Tschirnhaus and Leibnitz we now first learn from Gerhardt that it was as close as possible (see page 34, where this intimacy is already mentioned). Thus Leibnitz writes, petitioning favours and returning thanks to England; but when he has to do with his friends on the Continent he assumes the pretension of having no need of the English, calls himself the pupil of Huyghens only, and sneers at *Messieurs les Anglais*, while he is paying court to such people as Galloys.

If people will not admit the correctness of Boerhaave's expression as applied by Kant to Leibnitz's character, then they must aver, that Leibnitz was the most fortunate man in literary matters that the world has ever seen. For, although he corresponded with the original discoverer of the Differential Calculus, he invented it independently after it had been discovered, being in no way influenced or assisted by the fact that his bosom friend had a tract on it in his desk. Again, at a later period, when Leibnitz, at once desirous of concealing his discovery in reality, and of appearing to disclose it to the world, was requested by James Bernoulli to explain what he had or had not invented, this letter of Bernoulli's did not come into his hands till three years (!) afterwards, when Bernoulli had discovered by his own diligence that which Leibnitz chose not to tell him: and in 1689 Leibnitz wrote in the Acta Eruditorum *de motu corporum cœlestium*, without noticing Newton's Principia, in which this matter was treated of; the work having existed for the rest of the world since 1687, but for Leibnitz not till after he had given its contents, as discoveries of his own, in his Memoir. Thus Leibnitz must have made himself, if not purposely yet *de facto* Lord of Time, and if a fact took place too early for him he let it lie, and did not take it up until such time as suited him. Newton's letter of 24th October, 1676, was especially submissive and obedient to the fortune of Leibnitz, for not only did this letter not reach him, until after he had committed the Differential Calculus to paper as his own discovery, but in the interval (of nine months!) between the date of this letter and its delivery, Leibnitz was expressly asked whether it was his pleasure that it should come: *dites donc, si je dois vous bailler la grande lettre de Newton;* the person to whom it was entrusted, considering even a copy of this

letter so precious, that it could not be confided to the post, although the original was on every account to remain in London. Thus Leibnitz gained a considerable space of time, and had it in his power, when his own invention was quite ready to come out, to answer Newton in a grandiose style on the very day of the arrival of that nine-months'-old letter; *Accepi* [*hodie* (!)] *literas tuas sane pulcherrumas; e vestigio remitto inventum meum, quod a tuo, quod celâsti, non abludit.* How much less considerable would have been the glory, if the letter, instead of thus doing homage to the fortune of Leibnitz, had not enquired when it might be allowed to come. Now there are in the life of Leibnitz many such lucky incidents. Of a last quite trifling piece of good fortune,—that Leibnitz was able to make extracts from Newton's manuscripts, without Newton's knowing it, and that this, after the lapse of a century, can be so innocently narrated, and should not at all look as if it could not readily be explained as one of the miracles of the fortune of Leibnitz,—of this we need not speak. M. Gerhardt says that Leibnitz, without being obnoxious to any blame, could make extracts from his competitor's manuscripts.

NEW ADDITIONS AND REMARKS.

I will add in this English Edition a short remark on Oldenburg's position of the 28th October, 1676,—in reference principally to two letters of Newton, which are given by the Rev. Mr. Edleston, (p. 257, seq. *Com. Ep.* with Cotes,) and which are therefore here copied from Edleston's book:

NEWTON TO OLDENBURG.

Sr Octob 26. 1676.

Two days since, I sent you an answer to M. Leibnitz's excellent Letter. After it was gone, running my eyes over a transcript that I had made to be taken of it, I found some things wch I could wish altered, & since I cannot now do it my self, I desire you would do it for me, before you send it away.

In pag: 3. Sect: Pudet dicere.] for *a D. Barrow tunc Matheseos Professore* write only *per amicum*,

Pag: 5. Sect: At quando.] After *quibuscum potest comparari;* write *ad quod sufficit etiam hoc ipsum unicum jam descriptum Theorema si debitè concinnetur. Pro Trinomiis etiam et aliis quibusdam Regulas quasdem concinnavi* &c.

Pag: 6. Sect: Quamvis multa.] Where you find ye words *Gregorianis ad Circulum et Hyperbolam editis persimiles*, for *persimiles* write *affines*,

Pag: 9 or 10. Sect: Theorema de.] for *error erit* $\frac{v^3}{90} + \frac{v^4}{140} + \&c.$ write *error erit* $\frac{v^3}{90} + \frac{v^4}{194} + \&c.$

Pag: 6 vel 7. Sect: Quamvis multa.] about ye end of ye section turn *plenariam* into *plenam* or rather blot ye word quite out.

Pag: ult. vel penult. Sect: Ubi dixi]. write *solubilia* for *solutilia.* And if you observe any other such scapes pray do me y^e favour to mend them. So in pag 5 or 6. Sect. Quamvis multa.] It may be perhaps more intellig{ib}le to write *εὐθύνσει* for euthunsi.

Pag 8 or 9. Sect: Per seriem.] After y^e words *produci ad multas figuras:* you may if you please add these words, *ut et ponendo summam terminorum* $1 - \frac{1}{7} + \frac{1}{9} - \frac{1}{15} + \frac{1}{17} - \frac{1}{23} + \frac{1}{25} - \frac{1}{31} + \frac{1}{33}$ *&c esse ad totam seriem* $1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} +$ *&c ut* $1 + \sqrt{2}$ *ad* 2. *Sed optimus ejus usus &c.*

I feare I have been something too severe in taking notice of some oversights in M. Leibnitz letter considering y^e goodnes & ingenuity of y^e Author & y^t it might have been my own fate in writing hastily to have committed y^e like oversights. But yet they being I think real oversights I suppose he cannot be offended at it. If you think any thing be exprest too severely pray give me notice & I'le endeavour to mollify it, unless you will do it w^{th} a word or two of your own. I believe M. Leibnitz will not dislike y^e Theorem towards y^e beginning of my letter pag. 4 for squaring Curve lines Geometrically. Sometime when I have more leisure it's possible I may send him a fuller account of it: explaining how it is to be ordered for comparing curvilinear figures w^{th} one another, & how y^e simplest figure is to be found w^{th} w^{ch} a propounded Curve may be compared.

S[r] I am

Yo[r] humble Servant

IS. NEWTON.

Pray let none of my mathematical papers be printed w^{th}out my special licence.

Some other things in M. Leibnitz letter I once thought to have touched upon, as y^e resolution of affected æquations, & y_e impossibility of a geometric Quadrature of y^e Circle in w^{ch} M. Gregory seems to have tripped. But I shall add one thing here.

That y^{e} series of æquations for y^{e} sections of an angle by whole numbers, w^{ch} M. Tschurnhause saith he can derive by an easy method one from an other, is conteined in y^{t} one æquation w^{ch} I put in y^{e} 3^{d} section of y^{e} Problems in my former letter for cutting an angle in a given ratio, and in another æquation like that. Also y^{e} coefficients of those æquations may be all obteined by this progression

$$1 \times \frac{\overline{n-0} \times \overline{n-1}}{1 \times n-1} \times \frac{\overline{n-2} \times \overline{n-3}}{2 \times n-2} \times \frac{\overline{n-4} \times \overline{n-5}}{3 \times n-3} \times \frac{\overline{n-6} \times \overline{n-7}}{4 \times n-4} \times \&c.$$

The first coefficient being 1. y^{e} 2^{d}

$$1 \times \frac{\overline{n-0} \times \overline{n-1}}{1 \times n-1}. \quad y^{e}\ 3^{d}\ 1 \times \frac{\overline{n-0} \times \overline{n-1}}{1 \times n-1} \times \frac{\overline{n-2} \times \overline{n-3}}{2 \times n-2}. \ \&c.$$

& n being y^{e} number by w^{ch} y^{e} angle is to be cut. as if n be 5. then y^{e} series is $1 \times \frac{5 \times 4}{1 \times 4} \times \frac{3 \times 2}{2 \times 3} \times \frac{1 \times 0}{3 \times 2}$ that is $1 \times 5 \times 1 \times 0$ & consequently y^{e} coefficients 1.5.5. So if n be 6 y^{e} series is $1 \times \frac{6 \times 5}{1 \times 5} \times \frac{4 \times 3}{2 \times 4} \times \frac{2 \times 1}{3 \times 3} \times 0$ that is $1 \times 6 \times \frac{3}{2} \times \frac{2}{9} \times 0$ & consequently y^{e} coefficients 1.6.9.2. This scrible is not fit to be seen by any body nor scarce my other letter in y^{t} blotted form I sent it, unless it be by a friend.

For Henry Oldenburg Esq: *at his house*
about y^{e} middle of y^{e} old Pal-mall in
Westminster London.

NEWTON TO OLDENBURG.

S^{r}

I am desired to write to you about procuring a recommendation of us to M^{r} Austin y^{e} Oxonian planter. We hope yo^{r} correspondent will be pleased to do us y^{t} favour as as {*sic*} to recommend us to him, y^{t} we may be furnished wth y^{e} best sorts of Cider-fruit-trees. We desire only about

30 or 40 Graffs for y^{e} first essay, & if those prove for o^{r} purpose they will be desired in greater numbers. We desire graffs rather than sprags that we may y^{e} sooner see what they will prove. They are not for M^{r} Blackley but some other persons about Cambridge. But M^{r} Austin need only direct his letters to me or to M^{r} Bainbrigg fellow of o^{r} College. In y^{e} mean time we return o^{r} thanks to you & your friend for y$_{e}$ good will you have already shewn us.

M^{r} Lucas letter I have received, & hope to send you an answer y^{e} next Tuesday Post. I thank you for your care to prevent their prejudicing me in y^{e} Society, as also for giving me notice of y^{e} things miswritten in my late letter. In pag 3 y^{e} words you cite should run thus. Cujus triplo adde Log. 0. 8, siquidem sit $\frac{2 \times 2 \times 2}{0.8} = 10$. But in pag 8 y^{e} signes of y^{e} series $1 + \frac{1}{3} - \frac{1}{5} - \frac{1}{7} + \frac{1}{9} + \&c$ are rightly put two + & two − after one another, it being a different series from y^{t} of M. Leibnitz. But in y^{e} next two or 3 lines, to prevent future mistake you may if you think fit, after y^{e} words *res tardius obtineretur per tangentem* 45^{gr}, add these words *juxta seriem nobis communicatam*.

Seing y^{e} letter is still in yo^{r} hands, you will do me y^{e} favour to make these further amendments

Pag. 3 Sect [Pudet dicere] *cum D. Collinsio* for ad D. Collinsium

pag. 5. Exempl. 4 after y^{e} words *vel quibus libet dignitatibus binomii cujuscunq:* add *licet non directè ubi index dignitatis est numerus integer.*

pag 6 or 7 in y^{e} end of y^{e} section quamvis multa I desire you would cross out y^{e} words *adeo ut in potestate habeam descriptionem omnium curvarum istius ordinis quæ per* 8 *data puncta determinantur.* And in y^{e} 2^{d} sentence of y^{e} next section I could wish these words also *numero infinitè multas* were put out.

pag 9. Sect [*Præterea quæ.*] for *mihi quidem haud ita clara sunt* put *nondum percipio.* And after a line or two where you see y^e^ words *et certè minor est labor*, put out *certe.* XL.

By these alterations S^r^ you will oblige

Yo^r^ humble Servant

{Tuesday} Nov. 14 1676. Is. NEWTON.

From these two letters, and particularly the first, it becomes very probable, that on the 28th October, 1676, when Newton's letter of the 26th arrived, Leibnitz was actually in London. We know that Leibnitz was in London for "*one week in October;*" (Collins writes: "*Aderat hic Dom. Leibnitius per unam septimanam in mense Octobris;* "*in reditu suo ad Ducem Hannoveræ,*" see Collin's letter to Newton, dated 5th March, 1677, given in the *Commercium Ep.*, No. LXV;) and the word *send* in line 6 of Newton's first above cited letter of the 26th October, shows I think, that Newton at that time had no idea of Leibnitz's intention of visiting London, and no knowledge of Leibnitz's presence in London at that time. Now it is but fair to suppose that Oldenburg would have mentioned to Newton the personal appearance of Leibnitz in London, if not immediately, at least a few days after the fact.

Newton's not being aware of Leibnitz's presence on the 26th October, agrees well therefore with the supposition, that Leibnitz had not yet arrived in London, or had only just arrived at that date: the week of October spent by Leibnitz in London is hereby consequently proved to have been the last week of that month.

On the contrary, Newton's letter (of the 26th October) does not well agree with the supposition that Leibnitz's week in London could have fallen in an earlier part of October; for Newton would not, while he knew that Leibnitz was bound for a further journey, have spoken of sending the letter at once away; and would not, in his letter of the 14th November, line 26, have used the words "seeing" &c., if already, when he wrote his former letter, such knowledge had been in his possession.

We may therefore say that Leibnitz's week falls at the very end of October, which also agrees with Leibnitz's presence in Amsterdam on the 18th or 28th November, 1676, (his letter from Amsterdam bears that date—see *Com. Ep.*, loco cit.) and (agrees) with Guhrauer's words, "In October, 1676, Leibnitz quitted Paris, where it was not his fate "to return." (See Guhrauer's Life of Leibnitz, I. p. 170.)

Now, under this supposition, we think that Oldenburg may be excused for showing, nay, was almost obliged to show to Leibnitz Newton's above mentioned letter of the 26th October, and consequently also Newton's letter of the 24th October, intended for Leibnitz. That friendly disposition of Newton's, which Oldenburg is desired or permitted to testify to Leibnitz in the 34th line of the letter of the 26th October, could not have been better expressed to Leibnitz, than by Oldenburg's confidentially giving to him this letter; moreover in the 55th line Newton adds something intentively for Leibnitz.

Let any one reflect on Oldenburg's position, I do not say as a friend of Leibnitz, but as a friend of Newton, and I think it will appear to have been very natural, nay, only right perhaps, on the part of Oldenburg, to have shown to Leibnitz Newton's letter of the 26th October.

For Oldenburg was not a great Mathematician, and he had no reason to suppose that there could be any slight difference between what Leibnitz might be able to derive from Newton's letter to Leibnitz of the 24th, and what Leibnitz could get out of Newton's letter of the 26th October, nor is there perhaps any difference between the two.

But Leibnitz by reading the letter of the 24th and of the 26th October, was enabled to take a strong position for the purpose of pressing Oldenburg to show him Newton's manuscript *De Analysi*. For Leibnitz could with literal truth say, that the blotted condition of Newton's letter to him (see the last line of Newton's letter of the 26th) had prevented his reading it, and Leibnitz might infer from the 37th line of Newton's letter of the 26th, that it was only want of "*leisure*" that

had prevented Newton from giving other details (contained in the *Analysis*).

Here then the false position in which Oldenburg had put himself by showing the letter of the 26th brought him into a disagreeable dilemma, namely between refusing Leibnitz's request (to see the *Analysis*) bluntly, and without those excuses, which Newton had used in his letter to Leibnitz of the 24th: ("quoniam jam non possum explica-"tionem ejus prosequi,") and, on the other hand, complying with his request.

The excuse Newton pleaded was not an untruth in that higher sense of the excuse, in which Leibnitz (but not Oldenburg) was competent to view it; for Newton also, when speaking CONFIDENTIALLY (to Oldenburg) in his letter of the 24th October, had said: "I hope that this will satisfy M. Leibnitz, for, having "other things in my head, it proves an unwelcome interruption to "me to be at this time put upon considering these things." [See Edleston, p. LII.] But Oldenburg did not comprehend in what sense this excuse was meant, and must have half supposed that he was only requested by Leibnitz to do what Newton, if he had had the time, would have done himself. In this dilemma between having to say to Leibnitz more bluntly than Newton might wish, that something essential was kept back from Leibnitz, or else of overstraining the powers granted to him by Newton, Oldenburg erred, we think, by choosing the latter alternative, namely, of showing to Leibnitz Newton's manuscript *De Analysi.*

It may here be remarked that Leibnitz's so called invention of the new calculus, in his letter to Oldenburg of the 21st June, 1677, need not have appeared to Oldenburg (the word "*hodie*" being omitted) an act of piracy in regard to Newton, on account, I mean of Oldenburg's friendly act in showing him Newton's manuscript *De Analysi;* for Leibnitz had chosen for his invention the tangential side of the problem, which to Oldenburg must have appeared unconnected with his (Oldenburg's) friendly action.

We have already said in a former note, that the *nombreux voyages* of Leibnitz, which Messrs. Biot and Lefort mention, as explaining why Oldenburg did not sooner send Newton's letter of the 24th October to Leibnitz, are one of those French fictions, which those gentlemen introduce into the case; for Leibnitz arrived in Hanover in the latter part of December, [see Guhrauer, I. p. 188,] and it is with a bad grace that Oldenburg tells us, that the mere copy (!) of Newton's letter, (the original was to remain in London) could not have been sent before May, (four month's after Leibnitz's arrival in Hanover,) because the mere copy was in Oldenburg's eyes so valuable, that it (the copy!) could not go by post (!) though Newton's first letter had gone by post, and though Leibnitz, if he had not already secretly received a copy of the same in London, would, we suppose, have been in some small degree desirous to receive it soon, and might have friendly blamed Oldenburg, when he answered him, for keeping a copy of it so long out of his sight.

Oldenburg's position to Leibnitz indeed was not such, that Oldenburg should have hesitated to risk the very small trouble of having to get a second copy made, (which was the sole misfortune that could have ensued,) if by the will of God the first copy given to the post had been destroyed or lost.

Indeed we cannot believe this, nor need we believe it, for Oldenburg's little intrigue here in his own opinion was innocent.

All that we have said agrees well with Oldenburg's French postscript, in which he says, after having kept back the letter several months, "*dites donc si je dois vous bailler la grande lettre de Newton*," (and with Leibnitz's over hasty word "*hodie*" in his first draft of the letter, when at last he did answer).

It is interesting to add here some curious words of Leibnitz's answer to the *Commercium Epistolicum*, 1714, and some eager words of Newton, drawing consequences from Leibnitz's answer. We cite from *des Maizeaux's Recueil II.* page 5, seq., and Raphson's History of Fluxions, page 97, seq.

Leibnitz says, 1714: "Je fis connaissance avec Mr. Collins dans mon "second Voyage d'Angleterre——à mon second Voyage Mr. Collins me "fit voir une Partie de son Commerce; j'y remarquai que Mr. Newton "avoua aussi son ignorance sur plusieurs choses, et dit entre autres, "qu'il n'avait rien trouvé sur la Dimension des Curvilignes celèbres, "que la Dimension de la Cissoide."

Newton answers:

"Mr. Leibnitz instances in a Paragraph concerning my ignorance, "thinking that the editors of the *Commercium Epistolicum* omitted it, "and yet you will find it in the *Commercium Epistolicum*, page 74, "line 10, 11, and I am not ashamed of it. He saith, That he saw "this Paragraph in the hands of Mr. Collins when he was in London "the second time; that is, in October 1676. It is in my Letter of the "24th of October, 1676, and therefore he then saw that Letter."

What now does Leibnitz say? He is so far from denying that he had seen in London, in October, Newton's letter of the 24th October, 1676, that he actually asserts that he has seen at that time something more: "Comme "je n'ai pas," he says, answering Newton's words, "daigné lire le "*Commercium Epistolicum* avec beaucoup d'attention, je me suis trompé "dans l'Exemple que j'ai cité, n'ayant pas pris garde, ou ayant oublié "qu'il s'y trouvoit; mais j'en citérai un autre: M. N. avouoit dans "un des ses Lettres à M. Collins, qu'il ne pouvoit point venir à bout "des Sections secondes (ou Segments seconds) de Sphéroïdes ou corps "semblables: mais on n'a point inséré ce Passage ou cette Lettre dans "le *Commercium Epistolicum;* il auroit été plus sincere par rapport à "la Dispute, & plus utile au public, de donner le Commerce litteraire de "M. Collins tout entier, là ou il contenoit quelque chose qui meritoit "d'être lû; & particulierement de ne pas tronquer les Lettres, car il y "en a peu parmi mes Papiers, ou dont il me reste des Minutes."

Leibnitz died soon after writing this rather confused answer, but Newton was so much astonished at reading it, that he said in his "*observations*" upon the preceding epistle (Raphson, page 111, *des Maizeaux*, page 75):

"Mr. Leibnitz acknowledges, that when he was in London the second "time, he saw some of my Letters in the hands of M. Collins, especi- "ally those relating to *Series;* and he has named two of them which he "then saw, *viz.* that dated the 24th of October, 1676, and that in which "he pretends that I confessed my Ignorance of second Segments. And "no doubt he would principally desire to see the Letter which contained "the chief of my *Series*, and particularly that which contained those "two for finding the Arc by the Sine, and the Sine by the Arc, with the "Demonstration thereof, which a few months before he had desired "Mr. Oldenburg to procure from Mr. Collins; that is, the *Analysis per* "*æquationes numero terminorum infinitas.* But he tells us, etc."

Here we see that Newton, from the curious admissions of Leibnitz, began at last half to suspect that Leibnitz might have "*made extracts*" from his "Analysis." Gerhardt and Biot and Lefort now tell us that this half suspicion of Newton is well founded.

I take the liberty of copying here Edleston's *Synoptical view of Newton's Life*:—

1642 Dec. 25. ISAAC NEWTON born at Woolsthorpe, near Grantham, Lincolnshire.

1664 Feb. 19. Observations on two halos about the Moon.

1665 May 20. Paper on fluxions,* in which the notation of *point* is used.

Nov. 13. "Discourse" on fluxions and their applications to tangents and curvature of curves.†

1666 May 16. Another paper on fluxions.

Octob. Small tract on fluxions and fluents with their applications to a variety of problems on tangents, curvature, areas, lengths, and centres of gravity of curves.‡

Nov. Small tract similar to the preceding, but apparently more comprehensive.|| (Notation by *points* in first and second fluxions. Basis of his larger tract of 1671).

1669 July 31. His *De Analysi* sent by Barrow to Collins.

Dec. Writes notes upon Kinkhuysen's Algebra sent by Collins.

1671 July 20. Letter to Collins. (Prevented by a sudden fit of sick-

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* Shewing how to take the fluxion of (or to differentiate) an equation connecting any number of variables. It is referred to in a paper which seems to be part of a draft of his observations on Leibnitz's letter of Apr. 9, 1716. (Rigaud's *Appendix*, p. 23, compared with Raphson's *History of Fluxions*, p. 116).

† Rigaud and Raphson, *u. s.*

‡ In this tract his previous method of taking fluxions is extended to surds. The area of a curve, whose ordinate is y, is denoted by $\square\, y$. (Rigaud's Append. p. 23).

|| Raphson, p. 116. Wilson's Appendix to Robins' Tracts (II. 351—356).

ness from visiting him at the Duke of Buckingham's installation as Chancellor. Will not, he fears, have time to return to Discourse of infinite series before winter).

1672 May 25. Letter to Collins (does not intend to publish his lectures).*

Dec. 10. Letter to Collins, containing an account, requested by Collins in a letter received two days before, of his Method of Tangents.†

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* "Finding already, by that little use I have made of the press, that I shall not enjoy my former serene liberty till I have done with it, which I hope will be so soon as I have made good what is already extant on my account." He adds that he may possibly complete his method of infinite series, "the better half of which was written last Christmas." Macc. *Corr.* II. 322.

† This part of the letter is cited in the 3rd edition of the *Principia*, p. 246, instead of the letters to Leibnitz referred to in the two first editions. Its contents were sent to Leibnitz July 26, 1676, along with Newton's letter of June 13 of that year. There is a copy of it at the Royal Society (Miscell. MSS. LXXXI.) written in a tremulous hand, a consequence probably of the endeavour of the copyist to imitate Newton's writing. It has an address in Newton's hand, "These to his ever honoured ffriend Mr. John Collins...," and bears the post-mark of *May* 27 (probably 1676). This transcript may be conjectured to have been made at Collins's request for the purpose of accompanying the other papers which he was preparing to send through Oldenburg to Leibnitz. See *Commerc. Epist.* p. 47. (128, 2nd ed.) Doubts have been expressed whether these papers were actually sent to Leibnitz. We have however Collins's own testimony that they were sent as had been desired (*Comm. Epist.* p. 48, or 129, 2nd ed), besides Leibnitz's and Tschirnhausen's acknowledgements of the receipt of them. (*Ib.* pp. 58, 66, or 129, 142). It may also be observed that the papers actually sent (in a letter dated July 26, 1676) to Leibnitz by Oldenburg have been recently printed from the originals in the Royal Library at Hanover (Leib. *Math. Schrift.* Berlin, 1849), and that in them, as in Collins's draft, which is preserved at the Royal Society ("To Leibnitz the 14th of June 1676 About Mr. Gregorie's remains" MSS. LXXXI.), we find the contents of Newton's letter of Dec. 10, 1672, except that instead of the example of drawing a tangent to a curve, there is merely allusion made to the method. Collins's larger paper (called "Collectio" and "Historiola" in the *Commercium Epistolicum*), of which the paper just quoted "About Mr Gregories remains" is an abridgement, and which contains Newton's letter of Dec. 10 without curtailment, is stated in the second edition of the *Commercium* to have been sent to Leibnitz, but whether that was the case may be fairly questioned. This paper was intended by Collins to be deposited in the

1676 June 13. Letter to Oldenburg, containing a general answer to Lucas with a promise of a particular one, and also "some communications of an algebraical nature for M. Leibnitz, who by an express letter to Mr. Oldenburg had desired them." (read to the Soc. June 15: the part for Leibnitz* was sent to him at Paris, July 26).

Sep. 5. Letter to Collins. (Infinite Series of no great use in the numerical solution of equations. The University press cannot print Kinkhuysen's Algebra: the book is in the hands of a Cambridge bookseller with a view to its being printed: shall add nothing to it. Will alter an expression or two in his paper about infinite series, if Collins thinks it should be printed).

1676 Oct. 24. Latin letter to Oldenburg† for Leibnitz, who desired

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archives of the Royal Society, where it is still preserved, with the title "Extracts from Mr Gregorie's Letter" (MSS. LXXXI.), consisting of thirteen sheets. A copy of Newton's letter was sent to Tschirnhausen in May, 1675, in Collins's paper "About Descartes" (14 folio leaves, Roy. Soc. MSS. LXXXI).

* It was afterwards printed in Wallis's *Opp.* III. 622—629. (Oxf. 1699), and, from that work, in the *Commercium Epistolicum*, where the typographical error of 26 *Junii* for *Julii*, which is corrected in Wallis's *errata*, is also copied in the heading of the letter.

† The original letter extending over 14 folio pages is in the British Museum (MSS. Birch 4294). It was accompanied by a note to Oldenburg (Macc. *Corr.* II. 400) in a postscript to which he observes: "I hope that this will so far satisfy M. Leibnitz that it will not be necessary for me to write any more about this subject; for having other things in my head, it proves an unwelcome interruption to me to be at this time put upon considering these things." Newton sent some corrections by the next post (Appendix, p. 257). A copy of the Letter so corrected was not despatched to Leibnitz until May 2 of the following year, the delay arising from Oldenburg's anxiety to send this "Thesaurus Newtonianus" by a safe hand. Leib. *Mathem. Schrift.* I 1. 151 (Berlin, 1849).

On Nov. 14 he desired Oldenburg to make some further corrections, (Appendix, No. XVII.) which, however, were not introduced into the copy sent to Leibnitz, which was made ten days before.

This letter, like its predecessor of June 13, was printed in the 3rd Volume of

explanation with reference to some points in the letter of June 13.

Oct. 26. Letter to Oldenburg, with corrections for his letter of Oct. 24, &c.*

Nov. 8. Letter to Collins, thanking him for copies of the letters of Leibnitz and Tschirnhaus, with remarks shewing that Leibnitz's method is not more general or easy than his own.†

—— 14. Letter to Oldenburg (cider-fruit-trees: Lucas's 2nd letter: further alterations of his letter of Oct. 24).‡

We have omitted in this copy of Edleston's *Synoptical view* all those other valuable notes and dates which are irrelevant to our special subject. Messrs. Biot and Lefort can learn from what we give, how much an honest and elegant investigation in difficult matters differs from their sophistical and untrue pleadings.

Wallis's *Opera*, from which it was copied into the *Commercium Epistolicum*. Wallis says that he obtained his copies of the two letters from Oldenburg.

Leibnitz wrote two letters in answer (June 21, July 12, 1677); in the former of which he gives examples in differentiation. Oldenburg acknowledged the receipt of these Aug. 9, observing, "Non est quod dicti Newtoni vel etiam Collinii nostri responsum tam cito ad eas expectes, cum et urbe absint, et variis aliis negotiis distineantur." (Leibn. *Math. Schrift.* I. i. 167, Berlin, 1849). Oldenburg died the following month, but there is no reason to think that, if that event had not taken place, Newton would have departed from his intention of not continuing the correspondence. Leibnitz's answers will be found in Wallis's 3rd volume, the *Commercium Epistolicum* and his Works.

* Appendix, No. XVI.

† Macc. *Corr.* II. 403.

‡ Appendix, No. XVII.

In his papers on the early history of the Differential Calculus, particularly on Newton and Craig in the London, Edinburgh, and Dublin Philosophical Magazine of 1852, p. 321, Professor De Morgan makes the following statements: (the usual signs " " will denote that I use Professor De Morgan's words, while my own will be included in parentheses.)

"My present object [says Professor De Morgan] is the early "history of the *principle* of the Differential Calculus in England: "I mean the principle of infinitely small quantities, as distinguished "from that of ultimate ratios or limits."

Up to 1704 Newton always "used infinitely small quantities." The method of fluxions translated by Colson from Newton's latin, written in the period 1671—1676, is "strictly infinitesimal," and so also in the first edition of the Principia, 1687, the description of the fluxion "'is founded on infinitesimals.'" This will be seen in the following extract from the first edition of Newton:

"Cave tamen intellexeris particulas finitas. Momenta quam pri- "mum finitæ sunt magnitudinis, desinunt esse momenta. Finiri autem "repugnat aliquâtenus perpetuò eorum incremento vel decremento. "Intelligenda sunt principia jamjam nascentia finitarum magnitu- "dinum."

[We cannot, I think, agree with Professor De Morgan, who tells us, that these words of Newton are "strictly infinitesimal." On the contrary we see how Newton protests against these ideas].

"The treatise *De quadratura* was written by Newton long before "1704;—it appeared in its essential features in Wallis's Algebra of "1693—and here we now see the subsequent abandonment of uncloaked "infinitesimals." For Newton wrote,

1639 in Wallis:

"Quantitas infinite parva ... Et "hæ quantitates proximo temporis "momento per accessum incremen-"torum momentaneorum, evadent "$\dot{z} + o\dot{z}$ ...terminos multiplicatos per "O tanquam infinite parvos dele, et "manebit æquatio..."

and 1704 in his *De quadratura:*

"Quantitas admodùm parva... "Et si quantitates fluentes jam sunt "z, y, et x, hæ post momentum tem-"poris incrementis suis $o\dot{z}$, $o\dot{y}$, $o\dot{x}$, "auctae evadent $z + o\dot{z}$ ... Minuatur "quantitas o in infinitum, et neglec-"tis terminis evanescentibus..."

[Here Professor De Morgan thinks we see Newton's "subsequent "(1704) abandonment of uncloaked (1693) infinitesimals." But why has Professor De Morgan cut the phrases of Newton, and thereby prevented us from seeing that the word "fluentes," which he gives us (1704) occurs also in Newton's phrases (1693) in Wallis? With this word in 1693 we see much less of the *cloak* thrown over infinitesimals in 1704; the other little differences of expression are indeed inessential; at all events Newton did not throw (1704) a new borrowed cloak over his idea, but only perhaps somewhat more clearly than 1693 repeated his oldest idea of 1687: "principia (nascentia et) evanescentia."]

Professor De Morgan then speaks of Craig's book, and of Newton's behaviour in this respect; of Craig's and Newton's supposed clandestine measures and intentions, by which they wish to lead the public away from truth.

Craig, he intimates, wrote three separate tracts with almost the same title; the first was edited in 1685, the second in 1693, the last in 1718. "In the preface of 1718 Craig informs us, that in 1685 he "was a resident at Cambridge, and that Newton at his request read "his work. Here however we have reason to think, that he spoke "of the wrong tract; [why should, we ask Professor De Morgan, Craig, speaking on his own tracts, speak of the wrong one?] "After "some exemplifications of Barrow and Sluse, not referring to Newton "as having any method of quadratures, but only to the Binomial "Theorem, Craig proceeds to say, that nothing is wanting to extend "his method to all but transcendental curves, except only the removal

"of two difficulties. The first difficulty is the extraction of roots, "which he gets over by a Series of Newton's, which he hears that "Dr. Wallis has sent to press, [the first edition of Wallis is dated "1685] which Newton has had the goodness to communicate in manu-"script — — —."

The second of Craig's Tracts is of 1693. "That this was the tract," [though Craig in the Preface of 1718 says, that it was the Tract of 1685] "which Newton examined before it was printed, I infer as "follows. In the Preface of 1718, Craig states that Newton proposed "two curves, of which he gives the equations [these equations mentioned in the Preface of 1718 by Craig are: $m^2y^2 = x^4 + a^2x^2$, and $my^2 = x^3 + ax^2$] "as examples in corroboration of Craig's objections against Tschirn-"hausen." [Professor De Morgan withholds from us the knowledge, that in Craig's Tract of 1685 the first of these examples appears at page 42 in the form $Z^2 = \frac{(m^2 + x^2) \times x^2}{p^2}$].

"Now the attack upon D. T. (Tschirnhausen) is at the end of the "second tract of 1693, and the curves specified are the first two examples "at the beginning." [Professor De Morgan withholds from us the knowledge that Tschirnhausen is also attacked in the first edition of Craig of 1685, and not only attacked as in the second tract, but this with one of these very two equations, which Craig must consequently have received in 1685]. "Moreover in the *first* tract Craig was no "deeper in the Differential Calculus than to imagine, that $Pdy = Qdx$ "always gives $Py = Qx$, which we may undertake to say Newton could "not have passed over without detection." [Craig's book was not written on the Differential Calculus, but treated of quadratures as found by Craig's particular method; Newton had therefore no reason to speak to Craig of this method]. "It is also to be noticed, that in the second "tract the name of Newton does not occur once, though it is full of "the Differential Calculus, and though Leibnitz, Sluse, Barrow, Gregory, "are frequently mentioned. This, under all circumstances, we may "suspect was Newton's own doing." [Here Professor De Morgan

kills his enemy: Newton is not mentioned in the book; that must under all circumstances be Newton's doing! Newton is detected, as Professor De Morgan always detects him, playing a foul game. Newton had cleverly "cloaked over" with the cloak of fluxions and ultimate ratios Leibnitz's infinitesimals, and so Newton here again, as Professor De Morgan tells us, has hidden and cloaked himself over, for he must, as Professor De Morgan makes out, be in this book in which he is not mentioned. The lie is given to every witness before us, the truth being the contrary of what we read]. "And I am strongly inclined "to think that it was this very tract of Craig's, which immediately "suggested to Newton the progress which the views of Leibnitz were "making, and induced him to forward to Wallis the extracts from his "*De quadratura Curvarum.*" [There is in the date no difficulty]. "I conclude therefore that Newton, seeing the progress the Differential "Calculus was likely to make in England, procured the entire suppression "of his name in Craig's tract, and made up his mind to insert a part "of his treatise in the forthcoming work of Wallis."

So far we have principally cited Professor De Morgan; we shall now, in two words, give the fact as it actually stands. Craig did not as yet in 1685 understand either Leibnitz's Differential Calculus, of which only a little had transpired, or Newton's fluxional form, of which nothing till then was known, but he showed Newton something about another method of quadratures, and Newton for the sake of assisting Craig in an attack against Tschirnhausen, suggested to Craig two equations; one of which is still in our own days to be read in Craig's book printed 1685, although Professor De Morgan says that these examples were given 1693. In 1693 Craig had got hold, as he thought, of the whole theory of the Differential Calculus, and he here speaks a good deal of Leibnitz, but not of Newton; Newton indeed had in 1693 not yet published on the subject. But in 1718 when he wrote his third tract, Craig was fully acquainted with the discoveries of Newton and of Leibnitz, and he there says in his preface, that what he wrote in 1685, was for so early a date a good tract on quadratures, and he

was proud of saying that Newton saw this his first tract before it was printed in 1685. Every witness in the matter is here correct, with the exception of Professor De Morgan, who gives them all the lie. But they do not lie. Firstly Craig, speaking of his own tracts, says that it was his tract of 1685 that Newton saw. The tract itself, printed in 1685 (!) says that Newton gave the author a manuscript (see page 27!!). Professor De Morgan says no! it was your second tract in which Newton has assisted you. Now it happens that in this second tract Newton is not mentioned. For this very reason, says Professor De Morgan, it is this tract which Newton saw.

We ask, is it not absurd to treat history in this manner?

Professor De Morgan "*infers*" and "*supposes*" and "*is much inclined* "*to think*" that of all facts which are before us, the contrary is true, for Newton's honour is at stake. Now may we not "*infer*" and "*suppose*" and be "*inclined to think*" that Professor De Morgan is the last person whom we can trust in matters concerning Newton. This is very serious, for Professor De Morgan carries things at this moment in England with a high hand respecting Newton and respecting the history of fluxions, and clever Frenchmen eagerly avail themselves of his remarks.

We will add here the whole short preface of Craig's book of 1718, which says:

Præfatio ad Lectorem.

Habes hic B. L. *quæ multos ante annos de Calculo fluentium sum meditatus, & cujus prima Elementa, cum Juvenis essem, circa Annum* 1685 *excogitavi: Quo tempore* Cantabrigiæ *commoratus D.* Newtonum *rogavi, ut eadem, priusquam prælo committerentur, perlegere dignaretur: Quodq; Ille pro summa sua humanitate fecit: Nec-non ut Objectiones in Schedulis meis contra D.* D. T. *allatas corroboraret, duarum Figurarum Quadraturas sponte mihi obtulit; erant autem harum Curvarum Æquationes* $m^2y^2 = x^4 + a^2x^2$ *&* $my^2 = x^3 + ax^2$; *Meque interim certiorem fecit se posse hujusmodi innumeras exhibere per* Seriem Infinitam, *quæ in datis conditionibus abrumpens Figuræ propositæ Quadraturam Geometricam deter-*

minaret. In Patriam postea redeunti magna mihi intercedebat familiaritas cum Eruditissimo Medico D. Pitcairnio *& D.* D. Gregorio; *quibus significavi qualem pro Quadraturis Seriem haberet D.* Newtonus, *quam penitùs ipsis ignotam uterq; fatebatur. Post aliquot verò menses narrabat mihi D.* Pitcairnius *D.* Gregorium *Seriem similiter abrumpentem invenisse. Ego nullus dubitans, quin eandem ex duabus prædictis Quadraturis ipsi à me communicatis deduxerit, per Literas D.* Newtonum *rogavi, ut Seriem suam mihi transmittere vellet, ut an eadem esset cum* Gregoriana *perspicerem: Rogatui meo annuit Vir illustrissimus per Literas* 19 Sept. 1688 *datas: Nec mirum si parva esset inter utramq; Seriem discrepantia, cum* Gregorius, *ex duobus illis Exemplis & indicatâ a me Seriei* Newtonianæ *indole, suam facile deducere potuisset; quamq; statim in Tractatu D.* Pitcairnii De Inventoribus *publicandam curavit. Hanc historiolam Lectoribus impertire æquum videbatur, ut soli* Newtono *Seriem illam tribuendam esse cognoscerent. Satius quidem multo fuisset, si ipse (dum vivus esset)* Gregorius *eandem Orbi Mathematico communicasset, quodq; se facturum promisit per Literas dat.* Londini 10, Oct. 1691. *Me interim in iis hortatus est, ut, si quid haberem ad Memoriam ejus in hoc negotio juvandam, id ego quam citissime ad illum transmitterem; quod sine mora a me rem omnem fideliter ab initio narrante factum erat. Opus enim erat mihi facillimum, utpote qui omnes ejus &* Pitcairnii *Literas hanc materiam spectantes tum apud me habuerim, & adhuc habeo.*

Ego interim (ob plures rationes non jam enumerandas) nihil perquam generale in Quadraturis per hujusmodi Series expectandum fore ratus, ad propriam Methodum promovendam Studia mea convertebam: Nec irritos prorsus fuisse conatus colligere potes ex Tractatu Ann. 1693 *edito, & Specimine in* Actis Philosophicis *Anni* 1697 *de Spatiorum Transcendentium Quadraturis, quæ in* Geometria *omnino tum novæ erant. Ejusdem Anno* 1702, *longe ultra omnium aliarum limites promotæ, Theoremata aliquot generalia in* Act. Phil. *Anni* 1703 *erant publicata. Et magnopere mihi placuisse fateor, cum perciperem, quod prædicta Series* Newtoniana *Casum tantum simplicem Theorematis nostri primi comprehenderet. Integram jam Methodum cum aliis huic affinibus in sequenti libro*

explicatam B. Lector inveniet. Et si quidpiam in his ad Geometriam *promovendam sibi occurrat, tum me finem in his edendis propositum obtinuisse sciat.*

This is Craig's preface of 1718.

We will also give the first lines of Craig's second book of 1693, which are as follows:

"In actis philosophicis ——— specimen exhibui methodi generalis "determinandi Figurarum Quadraturas; cumque postea plus otii nactus "fueram, credebam me non posse illud meliùs, quam in eadem materiâ "ulterius perficiendâ, collocare; plurima enim tum deerant, quæque me "jam feliciter obtinuisse spero. Ne antem nimium mihi adscribere, vel "aliis detrahere videar, libenter agnosco Leibnitzii Calculum differentialem "tanta mihi in his inveniendis suppeditâsse auxilia, ut sine illo vix "assequi potuissem ———"

We add a remark, which we certainly cannot introduce without saying that it is a pity that Professor De Morgan has not found it out, for he would have made another use of it than we do.

It is supposed that Newton's "*De Analysi*," such as it was printed in the *Commercium Epistolicum*, and consequently such as it is now before us, was sent in 1669. But this perhaps is not the case.

The best witness in this matter is Oldenburg, especially if we attend to what he said in this respect in 1669, the very year in which Newton's treatise was written or sent to London.

Oldenburg's letter of 14th September, 1669, ad Franciscum Slusium, though inserted as No. XIII. in the *Commercium Epistolicum*, has not yet been read with attention respecting this question.

Oldenburg says at that date, that Newton's Analysis ("universalis "methodus Analytica") has been sent [to himself, to Lord Brounker, or to Collins]; Oldenburg then adds:

"Auctor sic incipit.

"*De Analysi per Æquationes numero terminorum infinitas.*

"Methodum generalem, quam de Curvarum quantitate per Infinitam "terminorum seriem mensurandâ olim excogitaveram, etc."

"Et— —ad calcem sic ait:

"Nec quicquam hujusmodi scio ad quod hæc Methodus, idque variis "modis, sese non extendat. Imo Tangentes ad Curvas mechanicas (si "quando id non alias fiat) hujus ope ducuntur. Et quicquid vulgaris "Analysis per æquationes ex finito terminorum numero constantes "(quando id sit possibile) perficit, hæc per Æquationes infinitas semper "perficiat.

"Et hæc de Areis Curvarum investigandis dictas ufficiant. Imo cum "Problemata de Curvarum Longitudine, de quantitate & Superficie

"Solidorum, deque Centro Gravitatis, possunt eo tandem reduci ut "quæratur quantitas Superficiei planæ linea curva terminatæ, non opus "est quicquam de iis adjungere."

Here we have the beginning and the closing words of Newton's *De Analysi*, such as Oldenburg had it before his eyes, 14th September, 1669.

Consequently what Oldenburg possessed cannot be that which has been given in the *Commercium Epistolicum*, for the latter only begins as Oldenburg says, but it does not end so.

We may suggest, that Newton's Analysis from page 83, line 18 in the *Commercium Epistolicum*, edition of 1722 [in Biot's edition page 67, line 15] was read some time in the year 1669 as follows:

Denique si index potestatis ipsius x vel y sit fractio, reduco ipsum ad integrum: ut in hoc exemplo $y^3 - xy^{\frac{1}{2}} + x^{\frac{4}{3}} = o$. Positio $y^{\frac{1}{2}} = v$, & $x^{\frac{1}{3}} = z$, resultabit $v^6 - z^3v + z^4 = o$, cujus radix est $v = z + z^2$, &c. sive (restituendo valores) $y^{\frac{1}{2}} = x^{\frac{1}{3}} + x$, &c. & quadrando $y = x^{\frac{2}{3}} + 2x^{\frac{4}{3}}$.

Et haec de Curvis Geometricis dicta sufficiant. Quinetiam curva, etiamsi Mechanica sit, methodum tamen nostrum nequaquam respuit.

"Exemplo sit Trochoides $ADFG$, cujus (etc.:" those 45 lines which are now read page 88, 89—in Biot page 71 [from line 10] 72 [to line 11]—up to the words "determinabilis est," with Oldenburg's finishing sentence as follows):

Determinabilis est. Nec quicquam hujusmodi scio, ad quod haec Methodus idque variis modis, sese non extendat (etc.: thirteen further lines just mentioned, page 118, as the end in Oldenburg's letter of 14th September).

We consequently believe, that page 84, 85, 86, (?) 87, and (?) page 90, 91, 92,9 3, (in Biot page 67, [from line 23] 68, 69, 70, [to line 8] 72, [to line 17] 73, 74, 75,) were introduced into the manuscript in or about the year 1672.

Newton, in fact, writing to Leibnitz 1676, 24th October, calls his treatise a "*compendium*," and says of it: "Eo ipso tempore quo (Mer"catoris Logorithmotechnia) prodiit, (1669) communicatum est ad D.

"Collinsium (meum) *Compendium* quoddam methodi harum serierum, in "quo significaveram Areas et Longitudines Curvarum omnium, et "Solidorum superficies et Contenta, ex datis Rectis; et vice versa, ex "his datis Rectas determinari posse— —deinceps Collinsius non destitit "suggerere ut haec publici juris facerem: Et ante annos quinque cum "suadentibus amicis consilium ceperam edendi Tractatum de Refractione "Lucis et Coloribus, quem tunc in promptu habebam; coepi de his "Seriebus iterum cogitare; et Tractatum de iis etiam conscripsi ut "utrumque simul ederem. Sed— —"

Newton here speaks of that Treatise (Tractatus) which is not in the *Commercium Epist.* but which was published by Colson 1736, and we see that "ante annos quinque" (that is 1672) he meditated on these matters.

We know that Newton was, after 1673 till 1683, engaged in labours of a different kind, and a copy of what we now read as Newton's Analysis has existed in Collins' handwriting, who died 1682. Therefore Newton's additions to what he first sent 1669 and what Oldenburg possessed 1669 were made at once in 1669 for Collins, or between 1669 and 1673, and this is what Collins possessed.

It is a pity, we repeat, that Professor De Morgan has not found out this fact; perhaps he will still think it not beneath his honour to make use of it in his fashion, and to draw from it with Anti-Newtonian instinct a good conclusion.

NEWTON'S SUPPOSED ERROR.

In a scholium at the end of the *Tractatus de Quadratura*, Newton says: "Quantitatum fluentium fluxiones esse primas secundas tertias "quartas, aliasque diximus supra. Hae fluxiones sunt ut termini serierum "infinitarum convergentium. Ut si x^n sit quantitas fluens, et fluendo "evadat $(x+o)^n$ deinde resolvatur in seriem convergentem

$$x^n + nox^{n-1} + \frac{nn-n}{2} oox^{n-2} + \frac{n^3 - 3nn + 2n}{6} o^3x^{n-3} + \text{etc.}$$

"terminus primus hujus seriei x^n erit quantitas illa fluens, secundus "nox^{n-1} erit ejus incrementum primum, seu differentia prima, cui "nascenti proportionalis est ejus Fluxio prima; tertius $\frac{nn-n}{2} oox^{n-2}$ erit "ejus incrementum secundum, seu differerentia secunda cui nascenti "proportionalis est ejus Fluxio secunda; quartus $\frac{n^3 - 3nn + 2n}{6} o^3x^{n-3}$ erit "ejus incrementum tertium seu differentia tertia, cui nascenti Fluxio "tertia proportionalis est; et sic deinceps in infinitum."

John Bernoulli caught hold of this, and wrote to Leibnitz (7th June 1713): "vides hanc——regulam (Newtoni) falsam esse. Nam excepto "primo et secundo termino, reliqui omnes alludunt a differentialibus "superioribus potestatis x^n——et hoc est, quod in nupero meo Schedi-"asmate Actis Lipsiensibus inserto jam notavi.——Animadverti New-"tonum in suo errore perseverasse usque ad annum 1711 cum libellus "ejus——fuit recusus. Sed in exemplari quod mihi dono misit per "Agnatum meum, ibi" (meaning that scholium) "calamo adscripsit

" altera vice voculam ut—ubi habebantur haec verba 'tertius (terminus) $\frac{nn-n}{2} o^2 x^{n-2}$ erit ejus incrementum secundum, et quartus

$$\frac{nn-3nn+2n}{6} o^3 x^{n-3}$$

" 'erit ejus incrementum tertium;' interseruit 'ut,' scribendo nunc, " 'erit ut ejus' etc."

In the *Acta Eruditorum* of Leipsig, Bernoulli made a great noise about this error of Newton. Montucla (in his *Histoire des Mathematiques III.* p. 105) speaking of the matter, says: "Bernoulli inséra dans les "actes de Leipsick, sous un nom déguisé une lettre fort amère, ou "Newton était peu ménagé; il y prétendait que Newton n'avait jamais "connu les régles de la seconde différenciation, ou celle de prendre la "fluxion d'une fluxion, et il se fondait sur ce que Newton dans son traité "de quadratura curvarum, dit, que les fluxions des différents degrès "sont représentées par les termes de son binome

$$x^m + [mx^{m-1}]\,\dot{x} + \left[\frac{m(m-1)\,x^{m-2}}{1.2}\right]\dot{x}^2 + \dots;$$

"or cela n'est vrai qu'en supprimant les dénominateurs numériques. "Car si l'on prend la fluxion, ou la différentielle, de mx^{m-1}, on aura pour "seconde différentielle seulement $[m(m-1)x^{m-2}]\,\dot{x}^2$. Mais il est évi- "dent, que c'était une pure inadvertence de Newton, séduit un instant "par une analogie qui regne entre sa formule et les fluxions successives. "Cette pièce (in the *Acta* of Leipzig) était si aigre, que Bernoulli a été "long temps sans l'avouer, mais il était facile de l'y reconnaitre."*

Now this famous Newtonian error, which almost Newton himself took for an error, writing the word "ut" with his pen in the copy of the second edition, when he presented the same to Bernoulli, (for Bernoulli

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* Montucla quotes the formula as x^m, etc. Bernoulli as z^m, etc. To make our remark clear, we have also in our quotation of Bernoulli written the letter x.

at the time and before he made his anonymous attack in the *Acta* of Leipsig, had through his nephew Nicholas Bernoulli, who made a journey to London, sent a friendly message on this matter to Newton), now we believe this famous Newtonian error is in truth no error.

The series

$$(x+o)^n = x^n + nx^{n-1}o + \text{etc.}$$

is, as is well known, Taylor's theorem in one of its cases. What we wish to say will therefore at first be in reference to this theorem.

The Differential or Fluxional Calculus has to solve the following problem and question: how great, if any (simple or complex) algebraic expression be augmented in only one of its constituent parts, is the augmentation of the whole expression, compared to the augmentation of the part. If, for instance, the algebraic expression (function) is (x^m), and it is the part x, to which we give an augmentation, and if we here again take the simpler case, by supposing the function (or algebraic expression) to be (x^2), then the question or problem is: how great is the augmentation of (x^2) if x receives a certain augmentation, which we will call dx?

It is false to suppose, that in a general manner the augmentation accruing to the whole by an augmentation of one part, can be indicated by one of the seven algebraic operations (addition, subtraction, multiplication, division, elevation into powers, extraction of roots, and logarithms.) On the contrary, the comparing of the augmentation of a whole function (or algebraic expression) to the augmentation given to a part of it, is such a complicated thing, that all the seven operations, and all their possible combinations are not able to make this comparison, and consequently not able to give us the answer.

This is the difficulty in which we are placed by the question. The Fluxional or Differential Calculus overcomes the difficulty, by introducing an eighth mode of algebraic operation, namely, by introducing to us first, the idea of a multiplication, such as ($a \times b$), to which secondly, we have to add this new idea, that, namely, the one of the two

factors or coefficients does not remain unchanged during the operation of the multiplication, but changes: is variable.

We ask: how is this possible? how can we multiply with a certain and clear result a by b, if one coefficient (let us say a) does not hold its value during the operation, being what is called a variable?

The Fluxional or Differential Calculus overcomes this new difficulty, which it has itself created, by telling us not only abstractly that the one coefficient (let us say a) is variable, but by telling us also which kind of variation there is in a.

In the above case, for instance, the algebraic expression or function being (x^2), and our question being: how great is the augmentation of (x^2) if x receives a certain augmentation, which we will call dx?—the answer given by the Fluxional or Differential Calculus tells us firstly, that the augmentation is a multiple of dx (of the augmentation given to the part of the function), namely, that it is $2x \times dx$, but with this understanding, that the one of the two factors or coefficients of this multiplication, namely, the expression $2x$, does not hold its value, but is variable, adding that the nature and kind of its variation is enunciated and indicated by the second differential, namely, that it is again a multiplication $(2 \times dx)\, dx$, which has to be considered as laying in the first.

In other words: since it is not possible to say by any common combination of the seven algebraic operations in a general manner (for a single case is not in question) what augmentation accrues to (x^2) when x (the one part of that function) is augmented by dx, therefore a new mode of operation is introduced, which is a multiplication in which the one coefficient holds in itself a second multiplication.

We repeat, the rule is: multiply dx with $2x$, which $2x$ flows during the multiplication, its fluxion being the augmentation of $2x$ when x is augmented.

This (eighth) mode of algebraic operation is, as we see, so complicated, that it cannot be expressed in one rule, but that two rules (or two

differentiations) are necessary, the first rule saying that the augmentation of (x^2) is dx multiplied with a thing, which is itself augmented, $[2x]$ and the second rule (or differentiation) saying, how this $2x$ is augmented.

If the function, or algebraic expression is simpler, namely, if it is addition or multiplication, for instance, if it is mx, then the Differential or Fluxional Calculus can say by one rule with one of the old seven operations what augmentation mx receives if one part of it (say x) is augmented by dx. Namely, the augmentation of mx is $m \times dx$. But if the function be (x^2), two rules are necessary, first and second differentials. The second rule is the correction of the first rule. The first rule speaks of the function, the second rule speaks of the first rule, and thereby indirectly of the function.

Sometimes the number of rules is infinite. For instance, the number of the rules for finding the augmentation of the function $(x^{\frac{1}{2}})$ or $\sqrt{x}$ is infinite, for after saying that that augmentation is dx multiplied with $\frac{1}{2}(x^{-\frac{1}{2}})$ $\left(\text{or with } \frac{1}{2\sqrt{x}}\right)$ wherein x is a variable, and after the second rule telling us, that the variability in that first rule is $-\frac{1}{4}(x^{-\frac{3}{2}})$, or $\left(-\frac{1}{4\sqrt{x^3}}\right)$, we still find this variable x in our answer, we must therefore give a third rule (third differential) respecting the variability of the second, which rule will again give us that variable x, and so on, without end.

In the case of (x^2) there is that end. Its first differential is $2x \times dx$ and the differential of this differential is $2dx \times dx$, where all coefficients and factorials are constants, for dx is (what few people seem to admit) a constant, whereas x is variable.

If we have till now before us the seemingly difficult idea of a multiplication regulated by a new multiplication, Taylor in his theorem brings new light into these ideas.

It is as if Taylor said: you may theoretically and in the abstract

speak of one rule (the first differential) and of its modification by a second rule (the second differential), and in your fancy you may think that one multiplication is hidden or lies in the other, but if you leave with me the world of abstraction and theory, and enter into nature and into reality, letting dx actually be something, a foot, or an inch, or some other reality according to the actual problem, then those abstract rules will require one very slight modification.

It is easy to show what this modification is.

We suppose there will be no possible objection if we say, that the series

$$1,\ 4,\ 9,\ 16,\ 25,\ 36\ldots$$

is one case of the variable function (x^2), namely, that case, in which the varied and variable part of the function (x) has been supposed to be in one instance 1, and in which the variability of x, or the augmentation dx given to x, is supposed to be also 1.

For in this supposition we have

$$\begin{aligned} x &= 1, \\ x + dx &= 2, \\ x + dx + dx = 2 + dx &= 3, \\ x + dx + dx + dx = 3 + dx &= 4, \\ &\vdots \\ &\text{etc.} \end{aligned}$$

Consequently in this case all the values which (x^2) can have, and therefore (x^2) itself (in the full extent of its variability) is contained in the series

$$\begin{array}{ccccccccccc} \ldots 1, & & 4, & & 9, & & 16, & & 25, & & 36,\ldots \\ & 3, & & 5, & & 7, & & 9, & & 11, & \\ & & 2, & & 2, & & 2, & & 2. & & \end{array}$$

We have put under the principal series of all these numbers their differences.

Thereby we see that it is not sufficient to give to x *one* augmentation dx, but that two such augmentations must be given and calculated before a difference of the first differences can appear. Between two

numbers of the series there is but one difference. Three numbers have two differences, and now (not when we have but two numbers) we can speak of a difference of the differences.

For instance, in our series the numbers 4, 9, 16 have the two differences 5 and 7, and now we can speak of the difference of these differences.

It is in this sense that Taylor, as it were, says: theoretically and abstractedly your rules can tell me that the difference of the function x^2 from its next augmented expression $(x + dx)^2$ is of a flowing nature, (not everywhere the same) and you can theoretically and abstractedly at once, in addition thereto, tell me, what the flowing nature (or fluxion) of that first difference is, but in reality we cannot find a second difference in *one* first difference, we can only find it in *two* first differences.

Therefore, Taylor wishing really to do in this function (x^2), or rather in all functions, what theory tells us, could not content himself with calculating one difference $[(x+h)^2 - x^2]$ or $[(x+dx)^2 - x^2]$, but finds himself obliged to calculate two successive differences $[(x+h+h)^2 - (x+h)^2]$ together with $[(x+h)^2 - x^2]$, before a difference of these differences (or the second differential) appears, and wishing nevertheless to give his formula only for *one* difference [only for: $f(x+h) - f(x)$], he consequently has to divide that member, which contains the second differential by 2.

We see that he consequently in his formula has to divide the member containing the third differential by 2×3, the following member by $2 \times 3 \times 4$, etc. Taylor's series contains therefore in truth, so as Newton said, the subsequent differentials: in other words, Taylor's or Newton's series is the Differential or Fluxional Calculus, and nothing else, and Taylor's series needs not to be proved, but proves the new Calculus, and is thereby proved itself.

Newton knew this, and in his constant practical applications and calculations from his youth till 1711, he had been so used to consider the terms of such series as the subsequent differentials, or fluxions practically, that also theoretically and abstractedly in the scholium which we have copied, he very properly said that they were both the same.

In 1711, when Newton was seventy years of age, he was (we may say this with the greatest veneration for Newton) not sufficiently intent on abstract mathematics to wish to dispute about the greater or smaller coincidence of the terms of those series with the subsequent fluxions; he therefore left the question open by adding the word "*ut*" to the scholium, which thereby is not essentially altered, not so much as it may appear altered by the word "*very*" in the third section of Newton's letter of 20th April, 1714 (in Edleston's *Corresp.*, page 173).

In a German treatise inscribed *Versuch die Differentialrechnüng auf andre als die bis herige Weise zü begründen*, I have explained this more at length.

Leibnitz, on his entrance into society, having just taken his degree at the University, became attached to the interests of Boineburg, who was on a small scale, we may say a Metternich of those ages, in the service of the Elector of Maintz. This Boineburg asserted from the time of the last election to the German Empire, a claim for money due to him from the King of France. The Elector permitted Leibnitz, who was in the Elector's service, to go to Paris, 1671, for the purpose of secretly and privately inducing the King of France to conquer Egypt, Leibnitz supposing that this scheme would avert the danger of the King's threatening position and preponderance in Europe. But the Elector did not pay for Leibnitz's stay in Paris, it being ingeniously arranged by Boineburg that this expense was charged to the King of France, whom Leibnitz had to advise and to persuade. Boineburg's own object was to realise through Leibnitz's presence in Paris his pecuniary claim. A strange errand (Leibnitz's moral "character" would necessarily be thereby affected; namely, "strengthened" and "advanced" Guhrauer says). Compare Guhrauer's Biography of Leibnitz (1846) I., page 49, line 10, et seqq.; page 52, line 12; page 55, line 1; page 56, line 17; page 59, line 12; (page 62, line 9, conf. page 82, line 6); page 95, line 3; page 98, line 26; page 105, line 27 ("entretenu"); page 125, line 12, etc.

No biographer or other author, I believe, has enquired: for what purpose did Leibnitz go to England towards the end of October, 1676? We presume that there were frequently in Havre or in Calais ships going viâ London to Amsterdam, and that the direct intercourse between France and Amsterdam was not common.

Perhaps some persons know of other reasons; to suppose him actuated by a desire to learn what mathematical secrets the English were yet withholding from the rest of the world, would suit this inquiry, but I have no grounds for believing it.

That Leibnitz was not in possession of the Differential Calculus on the 27th August, 1676, shortly before his second journey to London, is proved by his letter of that date, because in that letter he operates with the infinitely small quantity, called β, *vulgari more*, using the method of Transmutations, in the same kind of manner as not only Leibnitz, but everybody did at Leibnitz's time and before him.

Some passages of that interesting letter of the 27th August, 1676, are explained by a first draft of the same, which (imitating Gerhardt, who has edited first drafts of Leibnitz's several writings), we will give in extenso (the original is found in the bookcases, containing Leibnitz's manuscripts at Hanover):

(By using parentheses, such as [ ], we have indicated that Leibnitz

after finishing the letter struck out, what these parentheses contain, writing over the line what then follows):

This letter of the 27th August, 1676, which Leibnitz sent to England, is an answer to two letters, to one coming from Newton, and to another coming from Collins. His first draft has reference only to that part in which he answers Newton. We therefore, in what here follows on the left side of the page, have given *verbotenus* the first half of Leibnitz's letter, such as it went to England, and opposite to it on the right side, Leibnitz's sometimes longer, sometimes shorter first draft:

27 Aug. 1676.

Literae tuae, die 26 Julii datae, plura ac memorabiliora circa rem Analyticam continent, quam multa volumina spissa de his rebus edita. Quare Tibi pariter ac Clarissimis Viris, Newtono ac Collinio, gratias ago, qui nos participes tot meditationum egregiarum esse voluistis.	Literae tuae novissimae, Pellii Newtonii Gregorii Collinsii inventis et meditatis plenae plura ac memorabilia circa rem Analyticam continent, quam multa volumina ingentia impressa; vellem explicata essent, sed quomodo poterit id fieri in literarum angustia. Newtono et Collinsio multas a me gratias agas rogo;
Inventa Newtoni ejus ingenio digna sunt, quod ex Opticis Experimentis et Tubo Catadioptrico abunde eluxit.	Newtono quod Methodi suae circa series specimina mihi communicare voluit, plurimum me illi obligatum profiteor. Digna sunt omnia ingenio ejus, quod ex opticis experimentis et libro catoptrico abunde eluxit.
Ejusque methodus inveniendi Radices Æquationum & Areas Figurarum, per Series Infinitas, prorsus differt a mea: Ut mirari libeat diversitatem itinerum per quæ eodem pertingere licet.	Mirari vero subit in varietatem itinerum per quas perveniri potest ad interiora rerum; mea enim methodus longe a Newtoniana diversa est; in nonnullis ad eadem pervenimus; in pluribus alias plane

series[1] exhibeo. Universalitatem ex ipsa Methodi descriptione existimabitis, quam vobis exhibeo.

Mercator Figuras Rationales, Equidem fateor[2] Mercatori me

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[1] Newton's words "Method of Series" are here also used by Leibnitz, who calls not only one, but all his manifold methods, the method of "Series;" very properly, for quadratures etc. are, generally speaking, always given in infinite series, and are only exceptionally found in finite algebraic expressions.

[2] Leibnitz here says, that the invention which Newton's letter communicated to him (Newton's generalization of Mercator's divisions) had already before he received the letter been invented by himself, this being quite an easy matter.

This assertion of Leibnitz is not true, nor did he on second consideration venture to tell Newton this untruth, which we see in embryo only in this draft of the letter.

Leibnitz had just finished his work *De Quadratura* ("jam anno 1675 compositum habebam opusculum Quadraturae Arithmeticae, ab amicis ab illo tempore lectum," says Leibnitz, in *Act. Erudit.*, April, 1691, page 178). This he had completed in at least forty propositions in a beautiful manuscript ready for the press, which remains to this day in the Leibnitz shelves of the King's library in Hanover.

But in the same, written with a later hand, namely, not in Leibnitz's beautiful handwriting, in which the first part of this magnificent work is written, but in Leibnitz's hurried and quick handwriting, and squeezed in a narrow margin, we read the following:

"Scholium ad propositionem 29 quod ope progressionis Geometricae demon-"stravimus, poteramus et demonstrare per [aequationes] divisiones [pulcherrimas] "in infinitum continuatas pulcherrimas N. J. Mercatoris, Holsati, e societate Regia "Britannica——(quae) coincidunt cum prop. 26 si ponamus——prorsus etiam "expressiones (?) ac demonstrationes (?) propos. 28; deberet autem scribi istas esse "decrescentes. Simile quiddam ad radicum purarum vel affectarum extractiones "accommodari potest in numeris literisve, nam et in illis divisio quaedam locum habet, "quod jam dudum exemplis quibusdam expertus sum (ohe eos qui ex mea Circuli "expressione sequi putabant Circulum esse quadrato diametri commensurabilem) "etiam quantitates irrationales, *e.g.* diagonalem in quadrato per infinitam seriem "rationalium numerorum efferri posse. Sed hoc Clarissimum Virum Isaacum "Newtonum ingeniose ac feliciter prosecutum nuper accepi, a quo praeclara multa "theoremata expectari possunt. Porro si contra ponatur c aeq. AB^2 et b aeq. BF^2 "manente a aeq. BF^2 fiet $\frac{a}{b+c}$ aeq. $\frac{BF^2}{BF^2+AB}$ aeq $1-\frac{AB^2}{BF^2}+\frac{AB^4}{BF^4}$ etc., quemadmodum "ante aeq. $\frac{BF^2}{AB^2}-\frac{BF^4}{AB^4}+\frac{BF^6}{AB^6}$ etc., sive ponendo AB constantem sive permanentem,

seu in quibus Ordinatarum valor ex datis Abscissis rationaliter exprimi

[primam occasionem] partem deberi inventionum [Nam] circa series in-

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"aeq. 1, et BF vel BC aeq. t, tunc priore modo supra posito $\frac{FG}{2}$ sive $\frac{t^2}{1+t^2}$ fiet "aeq. $t^2 - t^4 + t^6$ etc., secundum propositionem 28 et summa omnium $\frac{FG^2}{2}$ sive area "dimidii spatii $BFG\beta$ erit $\frac{t^3}{3} - \frac{t^5}{5} + \frac{t^7}{7}$, et ad summam omnium $\frac{FG}{2}$ seu aream spatii "dimidii $BFG\beta$ mutatis mutandis evadit series $\frac{1}{1} - \frac{1}{1t} + \frac{1}{3t^3}$ etc., ut probari potest ex "corollario 2. ad prop. 25. ex quibus expressionibus per series cum t minor est "quam 1 prodierit series cum major est, et omnimodo sufficit prior sola, quoniam "si arcus BEM sit major quadr. 2 tunc sufficit comparari excessum EM."

So Leibnitz's great work *De Quadratura*, of which he speaks so often, was nipped in the bud by the first letter of Newton. For though in a short epitome of this Leibnitzian work, which Gerhardt has ventured to publish, the prop. 24 and 25, as Gerhardt admits, as well as the prop. 46, 47, 48, are of a later period, (see Gerhardt. Leibn. Math. Schr. V. Band 1858, page 86, line 27, and the note at page 105) still this later epitome could not efface all the weak points of this interesting work, and in particular not its twice repeated phrase: "oportet autem AB non esse minorem BF and oportet autem arcum BOD non esse quadrante majorem," (Gerh. l. c. page 107). So then Leibnitz finds, when Newton's letter arrives, that with Newton's invented general rules like those of Mercator, "poteramus" as he says, "demonstrare propositiones" 28, 29, etc., and that then these propositions would have been general, namely valid also, "si arcus major quadrante." On the whole, we see by that scholium that the work *De Quadratura* was disturbed and changed by what Leibnitz could take from Newton's letter "quam nuper accepi."

Consequently it is not true, that Newton's invention (the generalization of Mercator's divisions, etc.) was known and familiar to Leibnitz before Newton's letter; and not true, though Leibnitz uses the word "fateor," that he had taken "primam occasionem inventorum suorum" from this generalization of Mercator's idea.

Luckily only what Leibnitz wished to answer, not his actual answer, contained that untruth.

We beg Leibnitz's pardon for using such harsh language, for it is quite possible that Leibnitz was naïve, and cheated himself, believing in his having invented what Newton's letter contained, for some people (and Leibnitz may have been of these) having rather lively imaginations, find no distinction between what they learn and what they invent, but take some old idea of their own, which bears on the subject, for the mother of the new things which they learn, and then cheating themselves, say: "these are my children."

potest, (ut scilicet indeterminata Quantitas in vinculum non ingrediatur,) quadravit; & ad Infinitas Series reducere docuit, per Divisiones. *Newtonus* autem, per Radicum Extractiones.

finitas. Nam cum is Hyperbolam per infinitam seriem more suo [extricasset] quadrasset utique facile erat judicatu, posse quamlibet figuram rationalem eodem modo per infinitam seriem quadrari. (Another sentence appears to be written over this sentence above its lines, but very small, and to me unreadable and unintelligible.) Figura rationalis enim naturali aequatione explicari potest

$$\text{sit } y \text{ aeq. } \frac{x^3}{1+x^2} \text{ fiet}$$

$$x^3 - x^5 + x^7 - x^9 + x^{11} \text{ etc.,}$$

et summa omnium y seu area figurae erit

$$\frac{x^4}{4} - \frac{x^6}{6} + \frac{x^8}{8} - \frac{x^{10}}{10} + \frac{x^{12}}{12} \text{ etc.}$$

Vel etiam methodo[3] a Mercatoris diversa, nam si aliter dividas ex

$$\frac{x^3}{1+x^2} \text{ fiet } 1 - \frac{1}{x^2} + \frac{1}{x^4} - \frac{1}{x^6} \text{ etc.,}$$

[3] We are saturated through and through with the word "method" in Leibnitz's several writings, which occurs in this draft thirteen or eighteen times. Even this trifling remark on Mercator's divisions Leibnitz calls his method, and thinks that it differs from that of Mercator. Newton says in the Recensio (page 14 of the second edition of the *Com. Ep. Collinsii*, and page 17 in Biot and Lefort's edition) on a similar occasion: "Commercium cum Oldenburgio renovavit Leibnitius scribens se mirificum habere Theorema, quod daret Circuli vel ejus Sectoris cujuscunque Aream accurate in serie numerorum rationalium; Octobri autem insequente scripsit se invenisse Circumferentiam Circuli in serie simplicissimorum numerorum. Eadem Methodo, sic enim Theorema illud nominat:" Newton also then, as we see, was struck with Leibnitz's "methodo-mania" and with his desire of inventions, which made him always speak of "methods" when the thing itself was much more humble than what the word "methodus" would suggest.

et summa omnium ex cognitis Hyperboloeidium quadraturis habebitur. Sed quoniam plurimae figurae non sunt rationales, ut Circulus, Ellipsis, aliaeque innumerabiles, ideo opus erat methodo nova, qua demonstretur (?) figuras rationales inventas, esse aequales vel proportionales portionibus vel pendentibus figurae non rationalis quaesitae.

Mea methodus[4] Corollarium est

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[4] The note of the editors of the *Com. Ep.* here says: "*Leibnitius* hanc Methodum vulgari more prolixius hic exponit, quam Analysis ejus nova paucis exhibere potuisset, ideoque Analysin illam novam nondum invenerat."

"Hic modus transmutandi figuras Curvilineas in alias ipsis æquales, ejusdem est generis cum Transmutationibus *Barrovianis* & *Gregorianis*. Et Conicæ Sectiones hac Methodo semper ad Series Infinitas reduci possunt per divisiones. Generalis tamen non est: Nam si Curva sit secundi generis, incidetur in æquationem quadraticam; si tertii generis, in cubicam, si quarti, in quadrato-quadraticam, si quinti, in quadrato-cubicam, &c. præterquam in casibus quibusdam valde particularibus. Per extractiones vero Radicum Problemata facilius solvuntur absque Transmutationibus."

It should be remarked, that Leibnitz, in speaking of this invented method, here praises it, as reducing any high equations to equations "ubi dimensio ordinatae non ascendat ultra Cubum aut Quadratum aut etiam Simplicem dignitatem."

It is untrue that Leibnitz possessed such a method, and that he could reduce the higher equations into Cubic and Quadratic equations. The note of the *Com. Ep. Collinsii*, which we have just cited, remarks very quietly that the conic sections or equations of the second degree can be reduced thereby to a form applicable to Mercator's division, but that with this method "De Transformationibus" neither Leibnitz nor any one else could arrive at any result in the third nor in any higher degree, "praeterquam in casibus quibusdam valde particularibus." (It is in our days well known to mathematicians that sometimes a few particular cases of a higher order can be solved by the simplest "methods," to use Leibnitz's favourite word).

The note adds, with modesty: "per extractiones Radicum Problemata 'facilius' solvuntur absque Transmutationibus," which does not say, that all equations even of the highest degree could be squared and reduced by these Newtonian "extractiones radicum." Newton never pretended, either that his generalization of Mercator's divisions was his only method, or that it was already general by itself.

tantum doctrinæ generalis de Transformationibus; cujus ope Figura proposita quælibet, quacunque Æquatione explicabilis, transmutatur in aliam analyticam æquipollentem; talem ut, in ejus Æquatione, ordinatæ dimensio non ascendat ultra Cubum aut Quadratum, aut etiam Simplicem Dignitatem, seu Infimum gradum. Ita fiet ut quælibet Figura, vel per Extractionem radicis Cubicæ vel Quadraticæ, *Newtoni* more; vel etiam, methodo *Mercatoris*, per simplicem Divisionem; ad Series Infinitas reduci queat.

On the contrary, he says to Leibnitz in his first letter: "Quomodo ex Aequationibus sic ad infinitas series reductis— —cetera— —determinantur, et quomodo etiam Curvae omnes Mechanicae ad ejusmodi Aequationes infinitarum serierum reduci possunt — — longum foret describere: and: Non tamen omnino universalis evadit nisi per ulteriores quasdam methodos;" for this was what he did not wish to communicate in the letter, this being that part of the invention which is the Differential Calculus.

His generalisation of Mercator's divisions, together with his Differential Calculus, Newton, Wallis, Leibnits and everybody would at that time call the method of Series.

Moreover, in his second letter to Leibnitz, Newton gave (in enigmate) that part of his method which is the Differential Calculus, and there, in his second letter flinging out of his abundance a great number of theorems in 6 lines, (page 157, 173; Ed. of Biot, page 133) he modestly, but with some confidence says: "aliqua de his evadunt compositissima adeo ut vix per Transmutationem figurarum, quibus Jacobus Gregorius et alii usi sunt, absque ulteriori fundamento inveniri posse putem."

It is almost repugnant to us to read after these words again the second untruth of Leibnitz's letter, pretending that he could reduce all higher equations to cubic or quadratic or simple equations.

Indeed, if that was feasible, nobody needed to invent the Differential Calculus.

Ego vero, ex his Transmutationibus, Simplicissimam ad rem præsentem delegi. Per quam scilicet unaquæque Figura transformatur in aliam æquipollentem rationalem; in cujus æquatione, Ordinata in nullam prorsus ascendit Potestatem: Ac proinde sola *Mercatoris* Divisione per Infinitam Seriem exprimi potest.

Ipsa porro generalis Transmutationum methodus, mihi inter potissima Analyseos censenda videtur. Neque enim tantum ad Series Infinitas, & ad Approximationes; sed & ad solutiones Geometricas, aliaque innumera vix alioqui tractabilia inservit. Ejus vero Fundamentum vobis candide libereque scribo; persuasus quæ apud vos habentur præclara mihi quoque non denegatum iri.

Transformationis fundamentum hoc est: Ut figura proposita[6] rectis innumeris utcunque, modo secundum aliquam regulam sive legem ductis, resolvatur in partes; quæ partes, aut aliæ ipsis æquales, alio situ, aliave

[6] This sentence is very curious. Newton indeed must have smiled to see Leibnitz call "meam methodum" what he here describes, and what was extremely common, not only at that time but at any time, namely, to turn one figure into another figure by "*some means or other*," or, as Leibnitz has it, "*utcunque* (!) *secundum* "*aliquam regulam sive legem.*"

forma reconjunctæ, aliam componant figuram priori æquipollentem, seu ejusdem areæ; etsi alia longe figura constantem. Unde ad Quadraturas absolutas, vel hypotheticas Geometricas, vel serie infinita expressas Arithmeticas, jamjam multis modis perveniri potest.

Ut intelligatur; Sit Figura $AQCDA$. Ea, ductis rectis BD parallelis, resolvi potest in Trapezia ${}_1B\,{}_2D$, ${}_2B\,{}_3D$, &c. Sed, ductis rectis convergentibus ED, resolvi potest in Triangula $E\,{}_1D\,{}_2D$, $E\,{}_2D\,{}_3D$, &c.

Si jam alia sit Curva $A\,{}_1F\,{}_2F\,{}_3F$, cujus Trapezia ${}_1B\,{}_2F$, ${}_2B\,{}_3F$ sint Triangulis $E\,{}_1D\,{}_2D$, $E\,{}_2D\,{}_3D$ ordine respondentibus æqualia, tota figura $AE\,{}_3D\,{}_2D\,{}_1DA$, toti figuræ $A\,{}_1F\,{}_2F\,{}_3F\,{}_3BA$ erit æqualis.

Quinetiam Trapezia, Trapeziis conferendo, fieri potest ut ${}_1N\,{}_2P$, vel quod eodem redit, Rectangulum ${}_1N\,{}_2P$, sit æquale Trapezio respondenti ${}_1B\,{}_2D$, sive Rectangulo ${}_1B\,{}_2D$; tametsi recta ${}_1N\,{}_1P$ non sit æqualis rectæ ${}_1B\,{}_1D$, modo sit ${}_1N\,{}_2N$ ad ${}_1B\,{}_2B$ ut ${}_1B\,{}_1D$ ad ${}_1N\,{}_1P$; quod infinitis modis fieri potest.

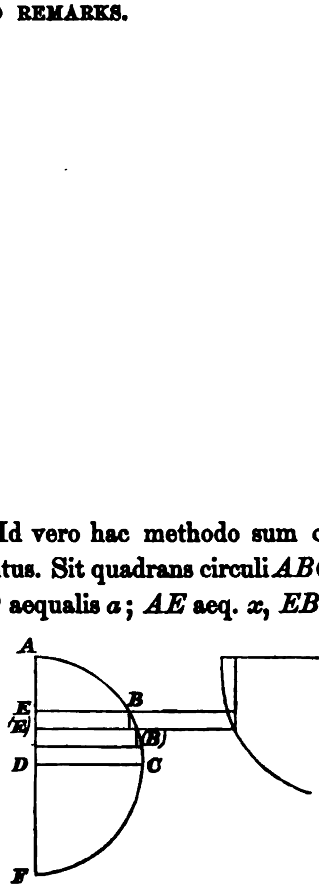
Quæ omnia talia sunt ut cuivis statim ordine progredienti, ipsa natura duce, in mentem veniant; contineantque Indivisibilium Methodum

generalissime conceptam, nec (quod sciam) hactenus satis universaliter explicatam. Non tantum enim Parallelæ & Convergentes, sed & aliæ quæcunque certa lege ductæ, rectæ vel curvæ, adhiberi possunt ad Resolutionem. Quanta autem & quam abstrusa hinc duci possint, judicabit qui methodi universalitatem animo erit complexus. Certum enim est omnes Quadraturas hactenus notas, absolutas vel hypotheticas, nonnisi exigua ejus specimina esse.

Sed nunc quidem suffecerit applicationem ostendere ad id de quo agitur; Series scilicet Infinitas, et modum Transformandi figuram datam in aliam aequipollentem rationalem, Mercatoris methodo tractandam.

AQCA sit Quadrans Circuli, Radius $AQ=r$, Abscissa $A{}_1B=x$, Ordinata ${}_1B{}_1D=y$, Aequatio pro Circulo $2rx-x^2=y^2$. Ducatur recta $A{}_1D$: producaturque donec ipsi QC etiam productae occurrat in ${}_1N$: Et $Q\,{}_1N$ vocetur z. Et erit $A\,{}_1B$ seu $x=\frac{2r^3}{r^2+z^2}$, et ${}_1B\,{}_1D$ sive $y=\frac{2zr^2}{r^2+z^2}$. Eodem modo, ducta $A\,{}_2D\,{}_2N$; si $Q\,{}_2N=z-\beta$ (posita scilicet ${}_1N\,{}_2N=\beta$) erit $A\,{}_2B=\frac{2r^3}{r^2+z^2-2z\beta+\beta^2}$;

Id vero hac methodo sum consecutus. Sit quadrans circuli *ABCD*, *AD* aequalis a; *AE* aeq. x, $EB=y$



erit aequatio pro circulo $2ax-x^2$ aeq. y^2. Haec aequatio in numeris resolvatur indefinite [methodo Diophantea] ut si ponatur: y aeq. $\frac{zx}{a}$ fiet aequatio $2a^3x-a^2x^2$ aeq. z^2x^2 sive $2a^3-a^2x$ aeq. z^2x vel x aeq. $\frac{2a^3}{a^2+z^2}$ aeq. *AE* et y aeq.

et $A\,{}_2B - A\,{}_1B$ sive recta ${}_1B\,{}_2B$, erit $\dfrac{2r^3}{r^2+z^2-2z\beta+\beta^2} - \dfrac{2r^3}{r^2+z^2}$ Sive, posita β infinite parva, (post destructiones et divisiones) erit ${}_1B\,{}_2B$

$$= \frac{4r^3z\beta}{\underline{|\,2\,|}\; r^2+z^2}.$$

Habita ergo recta ${}_1B\,{}_1D$, et recta ${}_1B\,{}_2B$, habebitur valor Rectanguli ${}_1D\,{}_1B\,{}_2B$, multiplicatis eorum valoribus in se invicem; habebitur inquam $\dfrac{8r^5zz\beta}{\underline{|\,3\,|}\; r^2+z^2}$, pro valore Rectanguli ${}_1D\,{}_1B\,{}_2B$.

Sit jam Curvae ${}_1P\,{}_2P\,{}_3P$ etc. natura pro arbitrio assumpta talis, ut Ordinata ejus ${}_1N\,{}_1P$ (ex data abscissa $Q\,{}_1N$ sive z) sit $\dfrac{8r^5z^2}{\underline{|\,3\,|}\; r^2+z^2}$. Ideo, quoniam ${}_1N\,{}_2N=\beta$, erit rectangulum ${}_1P\,{}_1N\,{}_2N$, etiam $\dfrac{8r^5z^2\beta}{\underline{|\,3\,|}\; r^2+z^2}$. Ac proinde aequale Rectangulo ${}_1D\,{}_1B\,{}_2B$ et spatium ${}_1P\,{}_1N\,{}_2N\,{}_2P\,{}_1P$ aequale spatio Circulari respondenti ${}_1D\,{}_1B\,{}_2B\,{}_2D\,{}_1D$. Est autem quaelibet Ordinata NP rationalis, ex data abscissa QN; quia, posita $QN=z$, Ordinata NP est

$$\frac{8r^5z^2}{\underline{|\,3\,|}\; r^2+z^2},$$

sive $\dfrac{8r^5z^2}{r^6+3r^4z^2+3r^2z^4+z^6}$. Ergo $\dfrac{2za^2}{a^2+z^2}$ aeq. EB. Sed quoniam ad aream Circuli habendam opus est summa omnium rectangulorum quale est $BE(E)$ et vero rationaliter invenimus ipsam y vel EB superest ut inveniamus ipsam $E(E)$ quod fiet subtrahendo AE ab $A(E)$ est autem AE aeq. $\dfrac{2a^3}{a^2+z^2}$ et $A(E)$ aeq. $\dfrac{2a^3}{a^2+(z)^2}$ ideo ponendo ipsas z indefinitas, pro arbitrio assumtas, esse progressionis arithmeticae, et differentiam omnium constantem, seu unam infinitesimam ipsius a esse, tunc sequens (z) erit $z-\beta$ ita ut differentia inter z et $z-\beta$ sit β, ergo $A(E)$ erit $\dfrac{2a^3}{a^2+z^2-2z\beta+\beta^2}$, et: $-AE+A(E)$ erit:

$$\frac{-2a^3}{a^2+z^2}+\frac{2a^3}{a^2+z^2-2z\beta+\beta^2},$$

aeq. $E(E)$ sive reductis omnibus ad unum denominatorem rejectisque illis, quae ceterorum comparatione sunt infinite parva, fiet $\dfrac{4z\beta a^3}{(a^2+z^2)^2}$ aeq. $E(E)$ quam quantitatem ducendo in EB aeq. $\dfrac{2za^2}{a^2+z^2}$ fiet $\dfrac{8z^2a^5\beta}{(a^2+z^2)^3}$ area rectanguli $BE(E)$. Cumque eadem sit ratio de ceteris id genus

ipsa per infinitam Seriem Integrorum exprimi potest, dividendo. Et Spatium talibus Ordinatis comprehensum, aequipollens Circulari, infinita Serie numerorum Rationalium, Methodo *Mercatoris* quadrari potest. Quod cum facillimum sit facere, hic omitto. Neque enim elegantiæ suæ, sed Methodi Generalis explicandæ causa, hoc exemplum assumpsi.

Ita siquis loco Circuli mihi dedisset Curvam, in qua Ordinata ascendisset ad gradum Cubicum, potuissem eam reducere ad Curvam, in qua Ordinata non assurrexisset ultra Quadratum, vel etiam ne quidem ad Quadratum.

rectangulis exiguis omnibus, patet summam infinitarum quantitatum (differentias infinite parvas habentium) quarum una est $\frac{8z^2a^5\beta}{(a^2+z^2)^3}$ daturam esse aream circuli, quare posito β esse ut diximus infinitesimam ipsius a, et ipsas z esse arithmetice proportionales, seu differentiam habentes constantem β, patet figuram curvilineam cujus abscissae sint z ordinatae vero $\frac{2z^2a^5}{(a^2+z^2)^3}$, futuram esse circulo aequipollentem quoniam ordinatae ejus in β (differentiam ipsarum z) ductae rectangulis[6] circuli elementaribus $BE\,(E)$ aequantur, ergo summa earum ordinatarum[7] in constantem β ductarum, seu area figurae curvilineae novae summae omnium eorum rectangulorum, seu areae portioni circulari respondenti aequabitur. Sufficit ergo invenire summam omnium $\frac{8z^2a^5}{(a^2+z^2)^3}$ seu omnium

$$\frac{8z^2a^5}{a^6+3a^4z^2+3a^2z^4+z^6},$$

quam fractionem Methodo Mercatoris in infinitorum paraboloëidum

[6] & [7] All these are not Leibnitian rules, but the well-known Theorems of Quadratures of Wallis and Cavalleri.

ordinatas resolvendo, earumque summas ipsis subjiciendo, habebitur series rationalis infinita exprimens magnitudinem semisegmenti seu portionis circularis ut *AEB*. Sit a aeq. 1, $8z^2$ aeq. b, $3z^2+3z^4+z^6$ aeq. c, fiet

$$\frac{8z^2}{1+3z^2+3z^4+z^6} \text{ seu } \frac{b}{1+c},$$

aeq. $b-bc+bc^2-bc^3$ etc.; et si tribus primis terminis contenti simus ipsasque b et c rursus explicemus fiet $\frac{b}{1+c}$ aeq. $8z^2-24z^4-24z^6-8z^8+12z^6+18z^8+158z^{10}+48z^{12}+8z^{14}$ etc. et ordinando $\frac{8z^2}{1+3z^2+3z^4+z^6}$ erit aeq. $8z^2-24z^4+48z^6+10z^8+158z^{10}+48z^{12}+8z^{14}$, etc. et summa omnium $\frac{8z^2}{1+3z^2+3z^4+z^6}$ erit aeq. $\frac{8z^3}{3}-\frac{24z^5}{5}$ etc. quae (si continuetur series modo praescripto) erit area spatii circularis *AEBA* posito *AE* aeq. $\frac{2a^3}{a^2+z^2}$ et *EB* aeq. $\frac{2za^2}{a^2+z^2}$. Atque haec est methodus generalis, quae omnibus omnino curvis analyticis, et suo modo etiam transcendentibus applicari potest, utcunque aequationes earum sint implicatae aut affectae, re ad puram analysin

Itaque semper, sive Extractionibus Radicum *Newtonianis* (gradus cujuslibet dati) vel Divisionibus *Mercatoris*, poterit cujuslibet Figuræ spatium inveniri, interventu alterius æquipollentis. Multum autem ad Simplicitatem interest quid eligas.

Omnium vero possibilium Circuli, & Sectoris Conici Centrum habentis cujuslibet, per Series Infinitas quadraturarum, simplicissimam hanc esse dicere ausim quam nunc subjicio.

Sit $QA\ _{1}F$ [*Vid. Fig. præcedent.*] Sector, duabus rectis in centro Q concurrentibus & Curva Conica $A\,_{1}F$, ad Verticem A sive Axis extremum perveniente, comprehensus. Tangenti Verticis AT occurrat Tangens $_{1}FT$. Ipsum AT vocemus t; & Rectangulum sub Semi-latere Recto in Semi-latus Transversum sit Unitas. Erit Sector Hyperbolæ, Circuli vel Ellipseos,

reducta: tantum enim opus est inveniri modum, quo aequalitas curvae naturam explicans rationaliter atque indefinite diophantes more solvatur; quod vero hic semper fieri potest secus ac in problematibus numericis, quoniam hic possunt irrationales etiam calculum ingredi modo ipsae indefinitae y et x in vinculis non comprehendantur.

Ista methodus generalis varios habet casus compendiaque innumera quae circulum examinanti sese obtulerunt, quorum unum, velut non inelegans ascribam, ipsius Harmoniae causa quam in ea deprehendo:

$$\begin{array}{lllllllllll}
\text{series } \frac{1}{3} & \frac{1}{8} & \frac{1}{15} & \frac{1}{24} & \frac{1}{35} & \frac{1}{48} & \frac{1}{63} & \frac{1}{80} & \frac{1}{99} & \frac{1}{120} & \text{etc.} = \frac{3}{4} \\
\frac{1}{3} & & \frac{1}{15} & & \frac{1}{35} & & \frac{1}{63} & & \frac{1}{99} & & \text{etc.} = \frac{2}{4} \\
& \frac{1}{8} & & \frac{1}{24} & & \frac{1}{48} & & \frac{1}{80} & & \frac{1}{120} & \text{etc.} = \frac{1}{4}
\end{array}$$

$$\left.\begin{array}{llll}
\frac{1}{3} & \frac{1}{35} & \frac{1}{99} & \text{etc.} \\
\frac{1}{8} & \frac{1}{48} & \frac{1}{120} & \text{etc.}
\end{array}\right\}\text{ex-}$$

$$\text{primit areae} \left\{\begin{array}{c}\text{circuli}\\ \text{hyperbolae}\end{array}\right\} \text{cujus quadratum inscriptum} = \frac{1}{4}$$

quod mutatis mutandis ad quaslibet etiam circuli portiones applicari potest. Quemadmodum etiam generalem habeo seriem pro area sectionis conicae centrum habentis cujuslibet, id est Circuli. Hyperbolae et Ellipseos per expressionem omnium ni fallor possibilium simplicissimam.

per Semi-latus Transversum divisus, $= \frac{t}{1} \pm \frac{t^3}{3} + \frac{t^5}{5} \pm \frac{t^7}{7}$ &c. Signo ambiguo $\pm$ valente $+$ in Hyperbola, $-$ in Circulo vel Ellipsi. Unde, posito Quadrato Circumscripto 1, erit Circulus $\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7}$, &c. Quæ expressio, jam Triennio abhinc & ultra a me communicata amicis, haud dubie omnium possibilium simplicissima est maximeque afficiens mentem.

Unde duco Harmoniam sequentem;

$\frac{1}{3}\ \frac{1}{8}\ \frac{1}{15}\ \frac{1}{24}\ \frac{1}{35}\ \frac{1}{48}\ \frac{1}{63}\ \frac{1}{80}\ \frac{1}{99}\ \frac{1}{120}$ etc. $= \frac{3}{4}$

$\frac{1}{3}\ \frac{1}{15}\ \frac{1}{35}\ \frac{1}{63}\ \frac{1}{99}$ etc. $= \frac{2}{4}$

$\frac{1}{8}\ \frac{1}{24}\ \frac{1}{48}\ \frac{1}{80}\ \frac{1}{120}$ etc. $= \frac{1}{4}$

$\left.\begin{array}{l} \frac{1}{3}\ \frac{1}{35}\ \frac{1}{99} \text{ etc.} \\ \frac{1}{8}\ \frac{1}{48}\ \frac{1}{120} \text{ etc.} \end{array}\right\}$ Exprimit aream

$\left\{\begin{array}{l} \text{circuli } ABCD, \\ \text{hyperbolae} \\ \text{aequilaterae} \\ CBEFC. \end{array}\right\}$ cujus quadratum inscriptum est $\frac{1}{4}$.

Numeri 3, 8, 15, 24, etc. sunt Quadrati Unitate minuti.

Eadem certis artibus ad curvas non analyticas sive transcendentes possunt applicari: [sed in] [ubi vero]: et methodum habeo propositâ longe generaliorem, de qua infra, per quam arbitror quantitatem incognitam possibilem determinatam quamcunque per seriem rationalem infinitam exprimi posse [quoniam] quamvis tam nominator quam numerator sit compositus.

Vicissim, ex Seriebus Regressuum pro Hyperbola hanc inveni. Si sit numerus aliquis Unitate minor $1 - m$, ejusque Logarithmus Hyperbolicus l, erit

$$m = \frac{l}{1} - \frac{l^2}{1 \times 2} + \frac{l^3}{1 \times 2 \times 3} - \frac{l^4}{1 \times 2 \times 3 \times 4} \text{ etc.}$$

Si numerus sit major Unitate, ut $1 + n$, tunc pro eo inveniendo mihi

Compendia autem reperi peculiaria pro regressu [ex arcu ad sinum aut sinum complementi, et pro regressu a logarithmo ad numerum] primum autem inveni regressum ex logarithmo ad numerum, ut inde etiam ab arcu ad sinum complementi. Easdem plane series inveni, quas in literis suis

etiam prodiit Regula, quae in Newtoni Epistola expressa est; scilicet erit

$$n = \frac{1}{1} + \frac{1^2}{1\times2} + \frac{1^3}{1\times2\times3} + \frac{1^4}{1\times2\times3\times4} \text{ etc.}$$

Prior tamen celerius appropinquat. Ideoque efficio ut ea possim uti, etiam cum major est Unitate numerus $1+n$. Nam idem est Logarithmus pro $1+n$ et pro $\frac{1}{1+n}$. Unde, si $1+n$ major Unitate, erit $\frac{1}{1+n}$ minor Unitate. Fiat ergo $1-m=\frac{1}{1+n}$, ac inventa m, habebitur et $1+n$, numerus quaesitus.

Quod regressum ex Arcubus attinet, incideram ego directe in Regulam quae ex dato Arcu, Sinum Complementi exhibet. Nempe, Sinus Complementi

$$= 1 - \frac{a^2}{1\times2} + \frac{a^4}{1\times2\times3\times4} \text{ etc.}$$

Sed postea quoque deprehendi, ex ea illam nobis communicatam pro inveniendo Sinu Recto, qui est

$$\frac{a}{1} - \frac{a^3}{1\times2\times3} + \frac{a^5}{1\times2\times3\times4\times5}$$

etc. posse demonstrari. Quod tribus Verbis sic fit. Summa Sinuum Complementi ad Arcum, seu omexhibet Newtonuspro regressu ex logarithmo ad amussim et pro regressu ab arcu ad sinum supplementi vel sinum versum, cujus differentia a radio est sinus complementi. Cujus [methodi vobis] compendii inventi demonstrationem tibi scribam, ut videas quam diversis rationibus ad eandem seriem venerimus: si sit numerus $1+n$ et logarithmus l erit n aeq. $\frac{l}{1} + \frac{l^2}{1,2} + \frac{l^3}{1,2,3}$ etc. quae series est in epistola gratissima Newtoniana, sed ego alia uti malo ejusdem originis, quae procedit per + et − alternative ac proinde celerius appropinquat. Nimirum quia idem est logarithmus pro numero $1+n$ ut pro numero $\frac{1}{1+n}$ hinc ponendo $\frac{1}{1+n}$ aeq. $1-m$ fiet m aeq.

$$\frac{l}{1} + \frac{l^2}{1,2} + \frac{l^3}{1,2,3} \text{ etc.}$$

unde facile ex invento m habebitur $1+n$ seu numerus Regressu utor ex arcu ad sinum complementi, nam posito arcu a radio 1 erit sinus complementi aeq.

$$\frac{1}{1} - \frac{a^2}{1,2} + \frac{a^4}{1,2,3,4} - \frac{a^6}{1,2,3,4,5,6}$$

etc. vel ut cum Newtono loquar

nium $1-\frac{a^2}{1\times2}+\frac{a^4}{1\times2\times3\times4}$ etc. est $\frac{a}{1}-\frac{a^3}{1\times2\times3}+\frac{a^5}{1\times2\times3\times4\times5}$ etc. Porro, Summa Sinuum Complementi ad Arcum (seu Arcui in locis debitis insistentium) aequatur Sinui Recto, ducto in Radium; ut notum est Geometris. Id est, aequatur ipsi Sinui Recto, quia Radius hic est Unitas. Ergo Sinus Rectus $=\frac{a}{1}-\frac{a^3}{1\times2\times3}+\frac{a^5}{1\times2\times3\times4\times5}$ &c. Hinc etiam, ex dato Arcu & Radio, sine ulla prorsus aliorum notitia, haberi potest Area Segmenti Circularis duplicati: quae est

$$\frac{a^3}{1\times2\times3}-\frac{a^5}{1\times2\times3\times4\times5}+\frac{a^7}{1\times2\times3\times4\times5\times6\times7}\ \&c.$$

Unde optime Segmentorum Tabula ad Gradus & Minuta &c. calculabitur.

Pro Trigonometricis autem operationibus, percommoda mihi videtur hæc expressio: Ut Sinus Complementi c ponatur

$$=1-\frac{a^2}{1\times2}+\frac{a^4}{1\times2\times3\times4};$$

quoniam sola, memoria retenta, (nam res eodem redit) sinus versus $\frac{a^2}{1,2}-\frac{a^4}{1,2,3,4}+\frac{a^6}{1,2,3,4,5,6}$ etc. Ex qua serie pro sinubus complementi facile demonstrari potest altera pro sinubus rectis a Collinsio nobis per Mohrium transmissa, ut postea animadverti, quoniam summa sinuum complementi ad arcum dat sinum rectum (ut facile demonstrari potest, et facile[*] ab illis deprehenditur, qui in his versati sunt) et summa omnium sinuum complementi ad arcum, seu omnium

$$1-\frac{a^2}{1,2}+\frac{a^4}{1,2,3,4}\text{ etc.}$$

$$\text{est } a-\frac{a^3}{1,2,3}+\frac{a^5}{1,2,3,4,5}\text{ ect.,}$$

ergo arcu posito a et radio 1 sinus rectus est

$$\frac{a}{1}-\frac{a^3}{1,2,3}+\frac{a^5}{1,2,3,4,5}\text{ etc.}$$

quamquam idem etiam recta consequi liceat, [quod initio non animadverteram.] Fundamentum autem demonstrationis talium omnium quae advidi simplicissimum est: exempli causa pro inventione numeri ex logarithmo

$$n \text{ aeq. } \frac{l}{1}+\frac{l^2}{1,2}+\frac{l^3}{1,2,3}\text{ etc.}$$

[*] These series of which Leibnitz here speaks with so much prolixity in 1676 are, as Newton shortly remarks in the Recensio (page 15, ed. of Biot and Lefort, page 18) the same which Leibnitz had received 1675 through Oldenburg.

omnibus casibus & operationibus, directis scilicet simul & reciprocis, sufficit; Quod ideo sit, quoniam Æquatio $c = 1 - \frac{a^2}{2} + \frac{a^4}{24}$ est plana. Unde si vicissim quæras Arcum ex Sinu Complementi, radix extrahi potest; adeoque fiet Arcus $a = \sqrt{6 - \sqrt{24c + 12}}$ exacte satis ad usum eorum qui in itineribus Tabularum commoditate carent; quia error æquationis non est $\frac{a^6}{720}$.

Innumera alia possunt dici, quæ his fortasse elegantia et exactitudine non cederent. Sed ego ita sum comparatus ut plerumque, Methodis Generalibus detectis, rem in potestate habere contentus, reliqua libenter aliis relinquam. Neque enim ista omnia magnopere aestimanda sunt, nisi quod artem inveniendi perficiunt, mentemque excolunt. Si quae obscuriora videbuntur, ea libenter elucidabo: Et illud quoque explicabo, quomodo hac methodo Aequationum quoque, utcunque affectarum, Radices per Infinitam Seriem dari possint, sine ulla Extractione; quod mirum fortasse videbitur.

Sed desideraverim ut Clarissimus Newtonus nonnulla quoque ergo summa omnium n est aeq. $\frac{l^2}{1, 2} + \frac{l^3}{1, 2, 3} + \frac{l^4}{1, 2, 3, 4}$ etc. ergo $n -$ summ. n aeq. l quaeritur ergo curva, in qua si ab n ordinata novissima assumta in unitatem seu parametrum constantem ducta, auferas summ. n seu aream figurae, residuum aequetur abscissae l in eandem a unitatem ductae, quam curvam certa analysi deprendetur solam ex omnibus possibilibus curvis esse Logarithmicam, ejusque constructione deprehendetur $1 + n$ esse numerum posito 1 logarithmo; simili methodo sinus complementi vel recti inventio ex dato arcu demonstrabitur nimirum in locum summarum substituendo summas summarum. Quae Methodus a Newtoniana ita longe lateque differt, ut mirer quomodo itinera usu adeo diversa eodem ducere potuerint vel uno in casu. Porro quoque cujuslibet aequationis sive finitae sive infinitae radicem methodo mea extrahere possum, finitae quidem, transformando problema Geometriae communis in problema tetragonisticum, cujus incognita semper infinita serie haberi potest; infinitae autem et finitae simul per quandam methodum non quidem

amplius explicet: Ut, Originem Theorematisquod initio ponit: Item, Modum quo quantitates p, q, r, in suis Operationibus invenit: Ac denique, Quomodo in Methodo Regressuum se gerat; ut, cum ex Logarithmo quaerit numerum. Neque enim explicat quomodo id ex Methodo sua derivetur.

Nondum mihi licuit ejus Literas qua merentur diligentia legere: Quoniam tibi e vestigio respondere volui. Unde non satis nunc quidem affirmare ausim, an nonnulla eorum quae suppressit, ex sola earum lectione consequi possum. Sed optandum tamen foret, ipsum ea potius supplere Newtonum: Quia credibile est, non posse eum scribere, quin aliquid semper praeclari nos doceat Vir (ut apparet) egregiarum meditationum plenus.

Ad alia tuarum literarum venio, quae Doctissimus Collinius communicare gravatus non est.

omnium simplicissimam, sed omnium generalissimam, quae hoc fundamento[9] nititur, quod datis duabus aequationibus finitis vel infinitis eandem incognitam continentibus, semper aequatio alia finita vel infinita reperiri potest in qua omnes dictae incognitae potestates sunt ablatae; quae methodus eo in casu servire potest, quo ceterae omnes deficiunt.

Habes origines eorum omnium quae a me in hoc argumento deprehensa sunt, candide prorsus et quantum sufficit illis qui nihil in his versati sunt expositas.

Saepe [Newtoni met.] porro saepe Newtoni methodus ad elegantiores ducet expressiones, saepe etiam mea ut res docet.

Multas alias habebam in ea ista meditationes et mittam (?) eas (?) quam primum Newtonus scire (?) poscit (?) nam etiam radicum extractiones per infinitas series coeperam et in affectis methodum Vietae

[9] This is Descartes' invention, his method of assuming an equation with undetermined coefficients, (see Schooten, the Geometria of Descartes, page 49, princ. 247, 262, and Gerhardt. Leibn. Math. Schrift. III. Band. page 727.) Leibnitz of course calls it his method ("mea methodus" hoc loco) because he just made use of it, and applied it (as Newton had done before him) to the newly-invented infinite series, (see Newton's letter of 24th October, 1676, [in literis transpositis] "altera tantum "in assumptione," etc.)

decimalem reddere nitebar generalissimam, idque me credebam omnium primum instituisse, sed aliud ex Newtoni literis didici non invitus.

Praecipitatam[10] vides epistolam tum quia responsum postulas, tum ne qua iniqua suspicione teneamini, quasi occasione [Newtoni] vestrarum literarum [adjutus fuerim] in hac re adjutus beneficium dissimulare voluerim; itaque gratas hodie, die lunae, in Germano pharmacopae redux domum forte praeteriens accipiens literas, nam lator earum, Regius, quem nominas, nondum domum meam invenerat, illis primo tabellione, ipso die mercurii, respondere volui.

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[10] Leibnitz we see likes to appear very prompt in replying when a letter from Newton arrives, "ne injusta suspicione teneamini quasi occasione Newtoni literarum adjutus fuerim."

He appeared so prompt in the most critical moment, namely when he answered Newton's second letter ("Hodie" accepi, as Gerhardt reads, Leibn. Math. Schr. I. Band, p. 154.)

It is quite true, as Newton says, that summations of infinitely small quantities having already been made by Wallis, and tangents drawn by Slusius in all those cases, where there was not any irrationality in the equation (in all hyperboloids and paraboloids)—the whole invention in fact depended upon carrying on one of the tangential methods through those cases where irrationalities occur.

Newton does this by taking in his work *De Analysi* $\sqrt{x}$ in the form of $x^{\frac{1}{2}}$ and $\frac{1}{x} = x^{-1}$ and saying, elegantly and most clearly, Regula I. si $ax^{\frac{m}{n}} = y$; erit $\frac{an}{m+n} x^{\frac{m+n}{n}} =$ Area. Quod exemplo patebit; si $x^2 + x^{\frac{3}{2}} = y$; erit $\frac{1}{3}x^3 + \frac{2}{5}x^{\frac{5}{2}} =$ Area. etc.[1] Again, in his letter of 10th Dec., 1672, he says: "mea methodus tangentium etc. non (quemadmodum Huddenii "methodus de maximis) ad solas restringitur aequationes illas, quae "quantitatibus surdis sunt immunes."

This was the invention, and Leibnitz did not make it, but he took it out of Newton's manuscript; for, supposing that all Gerhardt's documents are true, not one of them, in which $d\sqrt{x}$ occurs, is dated before Leibnitz's second journey to England.

The proof that Newton and not Leibnitz is the inventor, is therefore given by these documents, which may also serve to show us Leibnitz's conduct.

The first of the two Leibnitzian documents, namely, in which $d\sqrt{x}$ is

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[1] He thereby substituted a general calculation, in the place of isolated solutions.

mentioned, is dated November, 1676, and given by Gerhardt (Append. IV. to his tract of 1855). In this Leibnitz commits the fault of writing $d\sqrt{x} = \frac{1}{\sqrt{x}}$. On the whole Leibnitz is in this first document somewhat embarrassed with the new idea. But rejoiced at having mastered that form $\sqrt{x}$ he writes to Tschirnhaus some words, (which we have not) meaning that in Tamesis ostio, just after his second visit at Oldenburg's, he had got hold of a paradoxical idea.

We say Leibnitz wrote this because Tschirnhaus in a letter first published by Gerhardt, (Mathem. Schr. Leibn. IV. Band p. 431) begs Leibnitz to tell him what that idea might be, and at the same time, what the expected second letter of Newton might contain. The words in Tschirnhaus' letter, dated Rome 1677, are "quas series nescio num "per Methodum Gregorii possint terminari, et posses Dno Newtono "proponere saltem series hasce terminandas——methodum quoque, "qua haec inveniuntur, si desideras, sequentibus communicabo, nec "credo, qua es facilitate, sententias tuas paradoxas admodum, quas "eruisti in Tamesis ostio, nec non quaecunque se tibi memorabilia "offerunt celaturum. P. S. Endlich ersuche, so was würdiges in Mons. "Newton briefen mir zu communiciren" ("lastly, I beg that if in "letters of Mr. Newton there be anything remarkable, you will com- "municate the contents").

We have to notice, that although Tschirnhaus begged Leibnitz to tell him what the paradoxical idea might be, and at the same time what Newton's letters might contain, still not before 1679 did Leibnitz communicate to Tschirnhaus that he could master the form $\sqrt{x}$ (Gerh. ibidem, page 479: "sine sublatione fractionum et irrationalium—itaque "nunc opinor," compared with the end of page 470). Nor did Leibnitz ever communicate to Tschirnhaus Newton's second letter intended half for Tschirnhaus, nor his second answer to Newton, (Gerh. loco cit. p. 505, where Leibnitz makes reference only to his first letter to Newton).

So then "in Tamesis ostio" in the moment of departing from London, the paradoxical idea, the Differential Calculus, the pushing of the

rules for tangents through irrationalities, came to Leibnitz just after his second visit to Oldenburg as an extraneously learnt matter, namely with a mistake, Leibnitz writing $d\sqrt{x}=\frac{1}{\sqrt{x}}$. This is what the first document of Gerhardt contains, and Leibnitz at once spoke of this as of his own invention.

This is almost excusable here. For in Newton's Analysis, supposing that Leibnitz had inspected the same, there is no rule of tangents, or let us rather say tangents and their rules are in the Analysis everywhere, but still they are nowhere; they are in the paragraph "Longitudines "curvarum invenire," they are in the next following paragraph "In-"venire praedictorum conversum;" they are in the paragraph "Appli-"catio praedictorum ad Curvas Mechanicas," and in the words "Hinc in "transitu," etc. after the Demonstratio; but because they are everywhere and still have no particular place in the Analysis, therefore Newton added them to his Analysis in the letter of 10th December, 1672, in which he says "my method of tangents goes through irra-"tionalities," adding "hoc est unum particulare vel potius corollarium "generalis methodi quae extendit se ad omnia."

Now Leibnitz reading Newton's Analysis, saw how the letter of 10th December, 1672, which he had also read, was to be understood.

Leibnitz was puzzled with this for a little while, and at first fell into an error, but he afterwards succeeded. Something in the matter therefore is his own work. For the Analysis did not contain tangents, and the letter which did contain tangents, did not say how they evaded irrationality. Leibnitz therefore reading the Analysis had to deduce from it the tangential rule.

Now people may call this inventing, I call it the proof of a non-invention. For if in my letter which you clandestinely read, it is said I have the thing which is the great difficulty, namely to get over irrationalities, adding it is "una particula Methodi meae quae extendit "se ad omnia;" and if then you make extracts out of my Analysis in which my method is so extended "ad omnia," you are not the inventor

of my method, although you have just a little difficulty in adjusting my tangents[a] to my Analysis, to do which the geometers Oldenburg and Collins were not clever enough.

You may therefore, in some degree, think that you are the inventor, but you will have a certain disagreeable feeling within you, and will wish to avoid speaking of what you have seen.

Thus did Leibnitz avoid acknowledging that he had read Newton's letter of 10th December, 1672. Taxed with it, in the first edition of the *Commercium Epistolicum*, in the most conspicuous place of the *Com. Ep.*, namely in the last document, in the judgment of the Committee of the Royal Society, Leibnitz did not choose to answer; and his friends, including Professor de Morgan, deny that he had seen it till they are pushed into a corner by Edleston's new statements.

Also Gerhardt avoids speaking out clearly, for only hesitatingly does he tell us, that Leibnitz saw Newton's manuscript Treatise *de Analysi.*

[a] Gerhardt is quite mistaken, if he thinks the signs to be of consequence. On the contrary I will admit, that in scraps of Leibnitz's hand, dated before his second voyage to London, the signs Sdx and dx occur, as abbreviations, not as inventions. If we had Huyghens's or Wallis's scraps (as we have through Gerhardt those of Leibnitz) we might also in their calculations see, that in trying to find new quadratures, the calculator (we mean Huyghens or Wallis) would sometimes write down an abbreviation, perhaps dy or Sx^2, if at that state of the calculation it suited him, not to calculate what the sum or the difference (according to the nature of the formulæ) might be. But therewith no progress was made. Leibnitz and Wallis could not differentiate a single irrational form, not the form $\sqrt{x}$, and Wallis confessed, "hic haeret aqua." Irrationality occurs, unfortunately for Leibnitz and for Wallis, in all not quite elementary formulas, and that irrationality alone was to Wallis, as to all Geometers, the obstacle in their calculations. The signs S and d are therefore mere abbreviations if the theory had made no progress, and here it is proved by Gerhardt, that Leibnitz only just *after* his second voyage to London, and not before the same, learnt with difficulty to master this obstacle. He took Newton's general calculations out of Newton's Analysis, and therewith filled up (see the words "itaque nunc opinor" on p. 470, loco citato) those signs which were before but empty.

Almost, in the same manner, Leibnitz avoids Newton's name in the second document containing $d\sqrt{x}$, which Gerhardt gives us. In this second manuscript, namely, dated some months later, it is not the name of Hudde which is struck out, but that of Newton.

Compare Gerhardt's edition of this Leibnitian manuscript and Gerhardt's note to it, (both of which I give at the foot of this page*) and Daguerreotype Copies of its first lines, (which I have had taken in Hanover, and have deposited for inspection in Cambridge at the office of the Editor of this work, and in London, at Messrs. Macmillan and Co., Henrietta Street, Covent Garden.)

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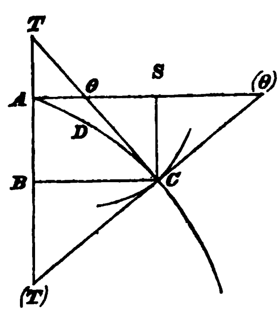
* The document is given in Gerhardt's Tract of 1855, page 143, in Appendix V., as being entirely in Leibnitz's hand-writing, and reads as follows:

11. Julii 1677.

Methode generale pour mener les touchantes des Lignes Courbes sans calcul, et sans reduction des quantités irrationelles et rompues.

Monsieur Slusius a publié la methode pour trouver sans calcul les touchantes des lignes courbes, dont l'equation est purgée des quantités irrationelles ou rompues. Par exemple une courbe DC estant donnée, dont l'equation exprime la relation de BC ou AS que nous appellerons y, à AB ou SC, appellée x, soit



$$a + bx + cy + dxy + ex^2 + fy^2 + gx^2y + hxy^2 + kx^3 + ly^3 \text{ etc.} = 0$$

on n'a qu'à écrire

$$\begin{aligned} 0 = b\xi + cv + dxv + 2ex\xi + 2fyv + gx^2v + hy^2\xi + 3kx^2\xi + 3ly^2v \\ + dy\xi \qquad\qquad + 2gxy\xi + 2hxyv \end{aligned}$$

It is thereby evident that Leibnitz knew that he had taken the Differential Calculus out of Newton's Analysis, and out of Newton's tangential letter; for, wishing to publish what he had so taken, in this second document of Gerhardt's, he struck out the name of Newton, and

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$$+ mx^2y^2 + mx^3y + pxy^3 + qx^4 + ry^4$$
$$+ 2mx^2yv + nx^3v + py^3\xi + 4qx^3\xi + 4ry^3v$$
$$+ 2my^2\xi + 3nx^2y\xi + 3py^2xv.$$

c'est à dire changeant l'equation en analogie:

$$\frac{\xi}{v} \doteq \frac{c + dx + 2fy + gx^2 + 2hxy + 3ly^2 + 2mx^2y}{b + dy + 2ex + 2gxy + hy^2 + 3kx^2 \text{ etc.}}$$

et supposant que $\frac{\xi}{v}$ exprime la raison $\frac{TB}{BC,\ x}$ ou $\frac{CS,\ y}{S\Theta}$, l'on aura TB, ou $S\Theta$, en supposant BC et SC données. Lorsque la valeur des grandeurs determinées b, c, d, e, etc. avec leur signes, fait de la valeur $\frac{\xi}{v}$ une grandeur negative, la touchante ne sera pas CT, qui va vers A commencement de l'abscisse AB, mais $C(T)$ qui s'en eloigne. Voila tout ce qu'on en a publié jusqu'icy, aisé à entendre à celuy qui est versé en ces matieres. Mais lors qu'il y a des grandeurs irrationelles ou rompues, qui enferment x, ou y, ou toutes deux, en ne peut se servir de cette methode, que par reduction de l'equation donnée à une autre delivrée de ces grandeurs. Mais cela grossit horriblement le calcul quelques fois, et nous oblige de monter à des dimensions tres hautes, et à des equations, dont la depression souvent est tres difficile. Je ne doute pas que ces Messieurs*) que je viens de nommer ne sachent le remede, qu'il y faut apporter, mais comme il n'est pas encor publié, et que je croy qu'il est connu de peu de personnes, outre qu'il donne la derniere perfection au probleme que M. des Cartes disoit avoir le plus cherché de tous les autres de la Geometrie, à cause de son utilité, j'ay jugé à propos de le publier.

Soit une formule ou grandeur ou equation, comme par exemple celle que dessus $a + bx + cy + dxy + ex^2 + fy^2$ etc. appellons la par abregé w et ce qui proviendra lors qu'elle sera traité comme ci-dessus: sçavoir $b\xi + cv + dxv + dy\xi$ etc. sera appellé $d\overline{w}$, de meme si la formule seroit λ ou μ, le provenu serait $d\overline{\lambda}$ ou $d\overline{\mu}$ et ainsi dans toutes les autres. Soit maintenant la formule ou equation ou grandeur w

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* Leibnitz hatte zu Anfang: Hudde, Slusius, et autres, geschrieben; spater hat er das Uebrige, ausser Slusius, durchgestrichen. ("Leibnitz having at first written: *Hudde, Slusius et autres*, struck this out, and left Slusius." Gerhardt's note, page 154, line 16).

while he said "je ne doute pas que ces Messieurs Hudde et Slusius" (of whom he knew that they could not get over irrationalities) "ne "sachent le remède, quand il y a des grandeurs irrationelles et rom- "pues," he had in his mind Newton, whose name he struck out, of

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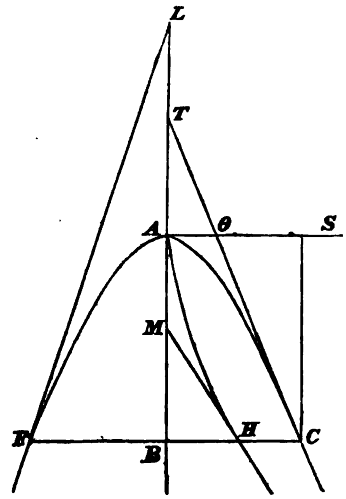
egale à $\frac{\lambda}{\mu}$, je dis que $d\overline{w}$ sera egale à $\frac{\mu d\overline{\lambda} - \lambda d\overline{\mu}}{\mu^2}$. Cela suffit pour manier les fractions

Enfin soit w égale à $\sqrt[s]{w}$, je dis que $d\overline{w}$ sera égale à $\dfrac{d\overline{w}}{\overline{s \sqrt[s]{w}^{\,s-1}}}$ ce qui suffit pour traiter comme il faut les grandeurs irrationelles.

Algorithme de l'analyse nouvelle de maximis et minimis, ou des touchantes.

Soit $AB = x$, $BC = y$, $T\Theta C$ la touchante de la courbe AC, et la raison $\frac{TB}{BC = y}$ ou $\frac{SC = x}{S\Theta}$ sera appellée $\frac{d\overline{x}}{d\overline{y}}$. Soyent deux ou plusieurs autres courbes AF, AH, et posant $BF = v$, $BH = w$, et la droite FL touchante de la courbe AF, et MH de la courbe AH, et $\frac{LF}{FB} = \frac{d\overline{x}}{d\overline{v}}$ et $\frac{MH}{BH} = \frac{d\overline{x}}{d\overline{w}}$, je dis que dy ou $d\overline{vw}$



sera égal à $vd\overline{w} + wd\overline{v}$; estant $v = w = x$ et $y = vw = x^2$, alors pour v et w substituant x, nous aurons $d\overline{vw} = 2xd\overline{x}$.

whom alone he knew this to be the case, and of whom alone it was true.

(Tout cela reussira aussi si l'angle ABC est aigu ou obtus, item s'il est infiniment obtus, c'est à dire si TAC est une ligne droite.)

This is Leibnitz's manuscript, as given by Gerhardt, which has to be compared with the Daguerreotype Copy. The same Daguerreotype contains a part of Leibnitz's first letter to Newton with almost the same apothecary-excuse mentioned above page 149. Gerhardt's words, which we give at the foot of page 155, refer to our page 155, line 20, and indirectly to what we have printed page 154, line 16, and the Daguerreotype Copy proves, that the name of Newton fell there in a somewhat particular manner out of the author's pen, though Gerhardt's note, which we have here given, has not spoken of the same.

The full length titles of the most modern works quoted by us in the present work are:

Sir David Brewster's Memoirs of the life, writings, and discoveries of Sir Isaac Newton. Edinburgh 1855.

J. Edleston's Correspondence of Sir Isaac Newton and Professor Cotes, including letters of other eminent men, now first published, etc. London 1850.

Leibnitzens mathematische Schriften, herausgegeben von G. J. Gerhardt. Erster Band. Berlin 1849. Zweiter Band. 1850. Dritter Band. Halle 1855 u. 1856. Vierter Band. 1858. Fünfter Band. 1859.

C. J. Gerhardt, Dr., die Entdeckung der Differenzialrechnung durch Leibnitz. Halle 1848. Cited as Gerhardt's I. (first) Tract or as Gerhardt's Tract of 1848.

Derselbe, die Entdeckung der höhern Analysis. Halle 1855. Cited as Gerhardt's II. (second) Tract, or as Gerhardt's Tract of 1855.

H. Weissenborn, Dr., die Principien der höheren Analysis, als historisch-kritischer Beitrag zur Geschichte der Mathematik. Halle 1856.

Other citations are indicated with sufficient precision in the work.

Gerhardt's Tract of 1855, p. 38, speaks of a book entitled, "*Gregorius* "*Vincentius Curvilineorum amœnior contemplatio, nec non examen circuli* "*quadraturæ. Lugd.* 1654." No such book exists, but only a "*Cur-* "*vilineorum amœnior contemplatio necnon examen circuli quadraturæ a* "*R. P. Gregorio Vincentio propositæ, Authore Vincentio Leotando;*" which book has not therefore, as Gerhardt makes out, Grégoire de St. Vincent for its author.

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